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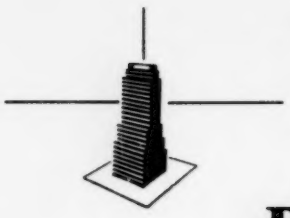

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THE AMERICAN PSYCHOLOGIST

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Volume 13 April, 1958 Number 4

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A THEORY OF EMOTIONALLY BASED DRIVE (*D*) AND ITS RELATION TO PERFORMANCE IN SIMPLE LEARNING SITUATIONS

KENNETH W. SPENCE

State University of Iowa

A NUMBER of years ago we instituted at the University of Iowa a series of experiments concerned with the role of aversive motivational factors in learning situations. In addition to the more usual direct manipulation of variables influencing the motivational state of an individual, such, for example, as varying the intensity of a noxious stimulus, degree of motivation was also varied in these studies by employing selected subjects who differed in terms of their performance on a so-called scale of emotional responsiveness or manifest anxiety (**31**). That these experiments have aroused considerable interest among both clinical and experimental psychologists is readily evident, not only from the large number of published studies that have attempted either to check or extend our experimental findings, but also from the not infrequent critical reactions they have elicited. Now, while some of the criticisms directed against our studies undoubtedly have merit, it has been rather dismaying to discover the extent to which many of them reflect a serious lack of understanding of the structure and purpose of the basic theoretical framework underlying the experiments.

While some of the responsibility for this failure to understand the nature and objectives of the theory can be assigned to the critics, I hasten to acknowledge that our theoretical treatments have been quite inadequate. The major difficulty is that the studies have appeared only in experimental journals in which space limitations have required that theoretical discussions be kept to a minimum. Since each article tended to limit the discussion to those portions of the theory relevant to the particular phenomena being reported, the theory has been presented only in a very piecemeal fashion. Apparently our hope that the interested reader, particularly the critic, would familiarize himself with the theory as a whole by considering all of the articles has not been realized.

THEORETICAL SCHEMA

One of the purposes of this paper is to provide a more systematic presentation of our basic theory, or, to use an expression recently introduced by Cronbach and Meehl (**4**), of the nomological network underlying our studies. Following this the experimental evidence bearing on the theory will be presented and discussed. Fig. 1 presents the main concepts employed, at least in so far as one kind of learning situation, classical conditioning, is concerned. At the top of the figure are shown the experimentally manipulated independent variables such as N, the number of paired conditioning trials; S_u, the unconditioned stimulus; ΣS_u, the number of previous presentations of the unconditioned stimulus; R_A, score on the anxiety or emotional responsiveness scale. The empirical response measure at the lower right-hand corner is the dependent variable. Inside the rectangle are represented the several theoretical concepts (intervening variables) and the interrelations assumed among them. The arrows indicate the functions relating the dependent response measure to the intervening variables, and the latter to the experimentally manipulated variables. Details of the portion of the theory between the intervening variable E and the empirical response measure (R_p), involving such theoretical concepts as oscillatory inhibition and response threshold, have been omitted since our present pur-

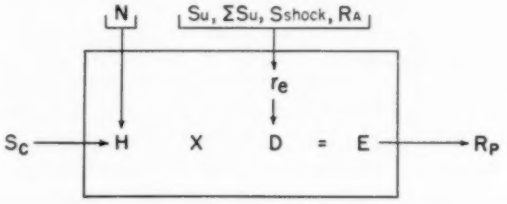

FIG. 1. Diagram representing portion of theoretical schema relevant to data for classical conditioning. (See text for explanation of symbols.)

pose does not require them. It is sufficient to state that response frequency (R_p) is some positive monotonic function of excitatory potential E.

That the schema presented in Fig. 1 conforms to the Cronbach and Meehl concept of a nomological net is readily apparent. Thus to quote these writers: "The laws in a nomological network may relate (*a*) observable properties or quantities to each other; or (*b*) theoretical constructs to observables; or (*c*) different theoretical constructs to one another" (4, p. 290). One may readily find examples in our schema of each of these "laws," or as I would prefer to call them, "relations," since the term "law" typically has a narrower meaning than these authors have given it.

The theory takes its start from Hull's basic assumption that the excitatory potential, E, determining the strength of a response is a multiplicative function of a learning factor, H, and a generalized drive factor, D, i.e., $E = H \times D$ (9). We have assumed, further, that the drive level, D, in the case of aversive situations at least, is a function of the magnitude or strength of a hypothetical response mechanism—a persisting emotional response in the organism, designated as r_e, that is aroused by any form of aversive stimulation. That is, aversive, stressful stimulation is assumed to arouse activity under the control of the autonomic nervous system, which, according to some neurophysiological evidence, may act as an energizer of cortical mechanisms. Those of you who are familiar with the theortical writings of Miller (13) and Mowrer (14) will recognize that this mechanism is similar to one these writers have postulated in connection with their investigations of acquired motivation. Thus they assumed that aversive stimuli arouse a hypothetical pain (emotional) response which, when conditioned to previously neutral stimulus events, provides the basis for an acquired drive of fear.

On the basis of analogy with overt reflexes to noxious stimulation, there were a number of properties that could be assigned to our hypothetical response mechanism. Three, in particular, will be discussed here. The first and most obvious is based on our knowledge that the magnitude or strength of observable reflexes to noxious stimulation (e.g., the corneal reflex to an air puff, the GSR to an electric shock) varies directly with the intensity or degree of noxiousness of the stimulus. Assuming our hypothetical emotional response, r_e, would exhibit the same property, it followed that the level of drive, D, present in classical defense conditioning would be a positive function of the intensity of the US. From the remaining portion of the theory, it could be deduced that the performance level, e.g., frequency of CR's, would vary positively with the intensity of the US employed. At the time of the original formulation of our theory there was some evidence, in particular an experiment by Passey (15), which supported this implication of the theory.

A second implication of our hypothetical mechanism was based on the adaptive property of observable reflexes to noxious stimuli: namely, that such responses characteristically exhibit adaptation or weakening with repeated stimulation. On the assumption that our hypothetical emotional response would behave in an analogous manner, it followed that, if a series of trials employing the US alone were given *prior* to conditioning, a lower level of D would be present during the subsequent conditioning than if no such adaptation trials were given. But if D were lower, the level of performance in the conditioning situation would also be lower following such adaptation trials than without them. This assumption or hypothesis, if you wish, is represented in Fig. 1 by $r_e = f(\Sigma S_u)$. At the time of formulation of the theory, we found a study by MacDonald (12) that gave results precisely in line with this implication.

The third implication of our theoretical mechanism was based on the well-known fact or observation that individuals differ in the magnitude of their reflex responses to a given intensity of stimulation. By analogy, again, we were led to assume that individuals would differ characteristically in the magnitude of this response, r_e, to the same intensity of stressful stimulation. If now there were available some means of assessing differences in this emotional responsiveness of individuals, our theoretical schema would lead to the prediction that highly emotional subjects, as assessed by the measuring device, would exhibit a higher level of performance in aversive forms of conditioning than subjects who scored low on the device.

The problem thus became one of attempting to develop a test for identifying individual differences in the responsiveness of this hypothetical emotional mechanism. Such a test, of course, would have to be defined independently of the measures that were to be employed in testing the theoretical network, i.e., the measures of performance in conditioning

and other learning situations. It was in connection with this portion of our theory that the Manifest Anxiety or A-scale was developed. The idea of using a self-inventory test that would differentiate subjects in terms of the degree to which they admitted to possessing overt or manifest symptoms of emotionality was suggested by Taylor in a doctoral dissertation (30).

At this point I should like to make a methodological digression and comment on a criticism recently made concerning this aspect of our research. One pair of critics, inspired, but unfortunately not too enlightened, by the excellent article of Cronbach and Meehl (4) on construct validity, insisted that we should have developed our scale for measuring *D* on the basis of a theory so that, and I quote them, "performance on it might be a basis for inferring drive (differences) independently of the outcome of subsequent experiments" (10, p. 162). While there are a number of highly questionable methodological points in the arguments of these critics, I should like to call attention here merely to the fact that it is simply not true that no theorizing guided us in the development of the A-scale. As has just been recounted, we did have some very definite theoretical notions as to what lay behind differences in level of generalized drive, *D*, especially in the case of classical defense types of conditioning.

This theory, that *D* is a function of the strength of the emotional response made by the organism to the noxious stimulation, had already received considerable support. Its extension in the present instance to the individual difference variable logically demanded that we measure the emotional responsiveness of individuals under comparable environmental conditions. Naturally, so-called physiological indices of emotionality, such as, for example, changes in pulse rate or in the GSR, were indicated; and we have conducted some research along this line. However, it occurred to Taylor that it might be both interesting and valuable to investigate the possibility of making use of the presumed behavioral symptoms of emotionality that clinicians have described. That the questionnaire type of test developed turned out as well as it apparently has is to the credit, I think, of the clinical psychologists who selected the behavioral items as indicative of emotionally over-reactive individuals.

In this connection a further comment is in order concerning a surprising question that was asked by these same critics. Thus the problem was posed as to what the consequences would have been for either the theory or the test had the experiments using the A-scale been negative. The answer to the question, at least regards the theory, should be obvious. The implications of the other portions of our theory with respect to our response mechanism, r_e, were sufficiently well confirmed that we would have had no hesitancy about abandoning the A-scale as being related to *D* in our theory. Since, however, the implications of this aspect of our theoretical net were confirmed, we have continued to employ the A-scale as one operational definition of this emotional responsiveness variable. That a more satisfactory scale, even one of this questionnaire type, can be developed, I have no doubt. Indeed, I would recommend that some of the time and energy now being squandered in the many distorted, even mendacious, criticisms that seem to find such ready acceptance in our current discussion-type journals be directed at this more constructive task. If the main purpose of these attacks is to discredit and eliminate the theory, they will fail in this objective, for the history of science clearly reveals that a theory is usually discarded only when a better theory is advanced. The same goes for the constructs within a theory.

EXPERIMENTAL EVIDENCE

With these methodological remarks out of the way, let us turn now to the experimental evidence bearing on our theoretical schema. I shall spend the major part of my limited space presenting and discussing the findings of our eyelid conditioning experiments, for it was in connection with data from this type of learning situation that the schema was originally formulated. With regard to performance curves of conditioning, e.g., frequency curves, the implications of the theory are, as we have seen, that level of performance will be a positive function of (*a*) the intensity of the US, (*b*) the level of score on the A-scale, and (*c*) the intensity of an extra stressful stimulation. We shall take up the first two of these variables together; since space is so limited, I shall present only those studies which had the largest sample of subjects and hence have provided the most reliable and stable data.

Eyelid Conditioning Experiments

In Fig. 2 are presented the findings of two experiments (one unpublished; the other, 27), one of

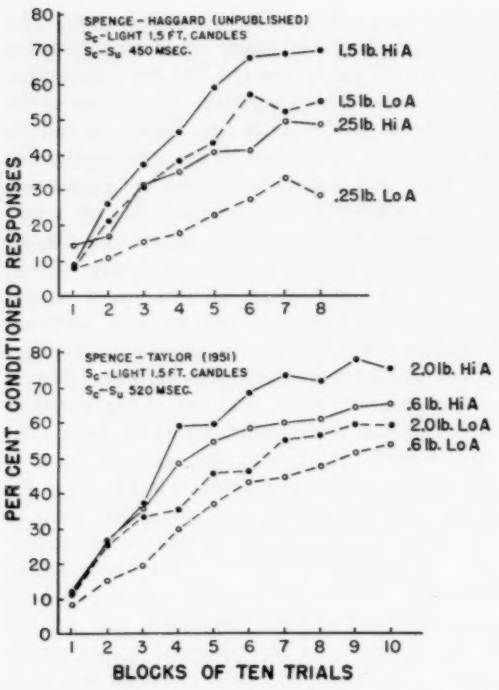
SPENCE - HAGGARD (UNPUBLISHED)
S_c - LIGHT 1.5 FT. CANDLES
S_c - S_u 450 MSEC.
1.5 lb. Hi A
1.5 lb. Lo A
.25 lb. Hi A
.25 lb. Lo A
SPENCE - TAYLOR (1951)
S_c - LIGHT 1.5 FT. CANDLES
S_c - S_u 520 MSEC.
2.0 lb. Hi A
.6 lb. Hi A
2.0 lb. Lo A
.6 lb. Lo A
PER CENT CONDITIONED RESPONSES
BLOCKS OF TEN TRIALS

FIG. 2. Performance in eyelid conditioning as function of A-score and intensity of US.

which involved 120 subjects and the other 100 subjects. Both studies employed two levels of puff intensity, .6 lb. and 2.0 lbs./sq. in., in the one represented in the lower graph; .25 lb. and 1.5 lbs./sq. in., in the upper graph. Each study also involved two levels of emotionality (upper and lower 20% of subjects on the A-scale). Examination of the curves in both graphs shows clearly that at each of the four puff intensities, the High A group (shown by solid curves) was well above the Low A group (broken curves). Statistical analysis over all of the conditioning trials revealed the differences were significant at the .01 level in the lower graph and at the .025 level in the upper.[1]

A second point to be noted in these data is relevant to the assumption that the learning or habit factor (H) and the drive factor (D) combine in a multiplicative manner to determine response strength. This assumption leads to the further implication that frequency curves of conditioning for different values of the anxiety variable will exhibit a gradual divergence over the course of training.[2] That this prediction was borne out may be seen by inspecting the graphs. Statistical confirmation of the divergence is revealed by the fact that the trials-×-anxiety interaction terms for both sets of data were highly significant (.005 and .025 levels).

The findings with respect to the intensity of US variable also supported the implications of our theory. Thus it may be seen in both studies that the subjects that had the strong puff performed at a higher level than those with the weak puff. The divergence between the curves is also apparent.

As an indication of the stability of our findings involving these two experimental variables, Fig. 3 presents data from these same two studies along with some relevant data from four other investigations recently conducted in our laboratory (26). Shown on the ordinate of this graph are the percentage of CR's given in the block of Trials 41–80 as a function of the intensity of the unconditioned stimulus employed. The uppermost curve in this graph represents the results for subjects selected from the high end of the A-scale; the lowest curve, subjects selected from the low end. The middle curve represents data obtained in four different experiments in which unselected subjects, so far as A-score, were conditioned under highly comparable conditions to those used with the selected subjects (i.e., similar visual CS and very comparable S_c–S_u intervals). The consistency of the results from experiment to experiment, particularly the relation of the curve for the unselected subjects to those for the High and Low A subjects, is, I believe, quite impressive.

In addition to the data presented in these graphs, four other investigations have reported finding that High A subjects responded at a significantly higher level than Low A subjects in eyelid conditioning (21, 22, 28, 30). One additional study (8) also found superior performance by High A subjects, although the difference in this instance was not significant. A reasonable interpretation of the failure to obtain a significant difference in this latter study, especially to anyone familiar with the variability

[1] Since unequal numbers of each sex were used in both of these studies and because women consistently exhibit a greater difference than men, the curves have been weighted equally for male and female subjects.

[2] This prediction must be qualified to the extent that the frequency measure has a ceiling of 100% and thus may not always reflect the continued growth of E. This is particularly the case at high levels of D in which E also is high.

of individual conditioning data, is that there were only ten subjects in each group.

Mention was made earlier of the fact that in addition to the anxiety scale we have also attempted to employ a number of physiological measures as further operational definitions of our emotional responsiveness variable. One of the most discouraging aspects of this work has been the lack of consistency, i.e., unreliability from day to day, of such measures. Especially has this been the case with the GSR, on which, unfortunately, we concentrated most of our time and energy. Recently, however, we have obtained results (to be published) with these measures that are rather more promising. Using changes in GSR and heart rate made to a mildly noxious stimulus and converting the measures into a so-called autonomic lability score by means of a formula suggested by Lacey (11), two groups of subjects who fell in the upper and lower third of the distribution of such scores were subsequently conditioned. Shown in Fig. 4 are the frequency curves of eyelid conditioning for these two groups of subjects. As may be seen, the subjects with the high autonomic lability index performed at a higher level than those with a low index. The difference is significant at the .02 level.

In addition to varying performance by manipulating the A-scale and US variables, it should be possible to produce a higher level of conditioning performance by presenting a strong extra stimulus, such as an electric shock, during the course of conditioning. Similarly, after a subject has experienced a strong electric shock just prior to conditioning, the mere threat of further shocks during the conditioning should arouse a strong and persisting emotional response that would raise the level of *D* and hence the level of performance. We have already published the results of one such experiment with unselected subjects which corroborated, in part, these theoretical expectations (25).

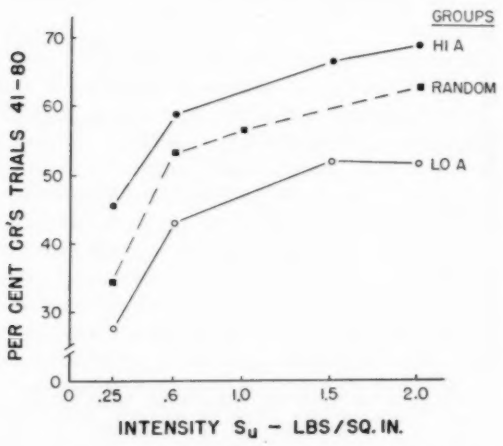

FIG. 3. Showing relation between conditioning performance and intensity of US for unselected and high and low A-score subjects.

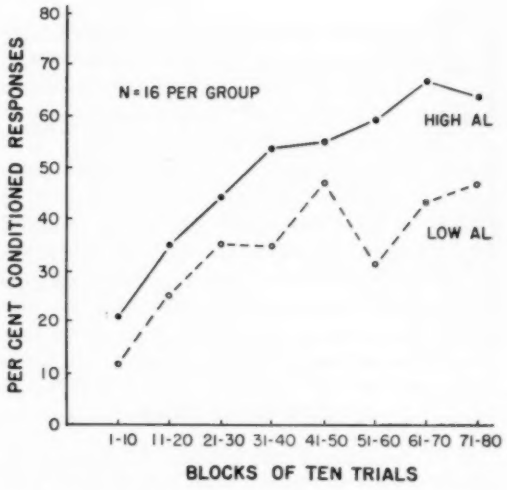

FIG. 4. Percentage of CR's for the high and low autonomic lability (AL) groups in blocks of ten trials.

Recently a further experiment (to be published) studying the effects of shock threat on High and Low A subjects was completed in our laboratory. Some idea of the nature of the findings can be gained from Fig. 5 which presents the frequency curves of conditioning for four groups of Low A subjects (20th percentile). The two top curves in this graph are for subjects conditioned with a relatively strong air puff (1.5 lbs./sq. in.); the lower two, for subjects who had a weak puff (.25 lb./sq. in.). It will be seen that at both puff levels the threatened group (solid curve) was consistently above the nonthreatened group (broken curve) throughout the whole 80 trials. A similar experiment with high anxious subjects revealed a difference between the threat and nonthreat groups throughout the conditioning in the case of groups which had a weak puff. In the strong puff groups, however, the curves for the threat and nonthreat group, after separating in the early trials, came together in the later stages of the conditioning (last

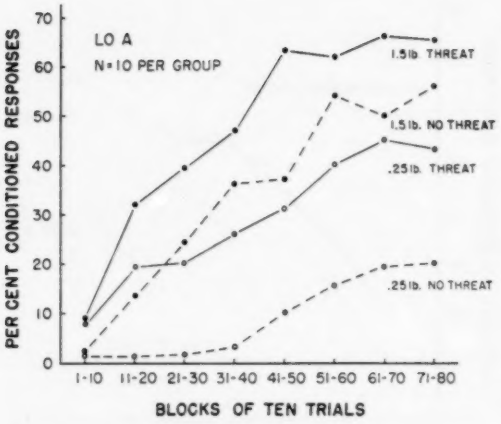

FIG. 5. Percentage of CR's in blocks of ten trials as a function of shock or no-shock threat and intensity of the US for subjects who score low on A-scale.

40 trials). This latter effect undoubtedly results, in part, from the ceiling imposed by the frequency measure.

Space will not permit a detailed presentation of the experimental evidence with respect to that portion of our theory concerned with the assumption that the emotional response to the noxious US would be weaker if adaptation trials are given prior to conditioning. It is sufficient to state that the original finding of MacDonald (12), that such preadapted subjects exhibited a lower level of performance in conditioning than nonadapted subjects, has been corroborated by Taylor (32). The latter experimenter also found that conditioning performance was inversely related to the intensity of the US employed during the preconditioning adaptation period. Thus the implications of this part of the theoretical network have also received further support.

The final set of conditioning data that I would like to present are concerned with the effect of level of *D* on differential conditioning. Without going into the theoretical derivation, it may be shown that one of the implications of our theory is that the higher the drive level, *D*, of the subjects, the greater should be the differentiation between the positive and the negative, i.e., nonreinforced, stimulus in such differential conditioning. Two studies from our laboratory have reported finding that, in five separate comparisons, high anxious subjects showed better discrimination than low anxious subjects (21, 23). Although none of the differences were significant, four closely approached being so. More recently we have investigated the effect of varying the level of *D* on differential conditioning by direct manipulation of the intensity of the US. The graph in Fig. 6 presents the findings of this study (17) in terms of the frequency of CR's given to the two stimuli, positive and negative. As may be seen, the subjects conditioned with the strong US not only showed a higher level of response to the positive and negative stimuli, but, as predicted from our theory, the difference between the conditioned responses to the two stimuli was greater in the case of the group that had the strong US. Again this latter difference approached, but did not quite reach, statistical significance. Unfortunately, conditioning data are plagued by high individual variability, produced in part by a few subjects who show very little or no conditioning. In an effort to ascertain what the finding would be for subjects who showed considerable conditioning, a separate analysis was made of the upper two-thirds of each of the two groups run in the experiment. In the case of these subjects discrimination was significantly better for the high drive group at the .05 level.

So much for the findings in our eyelid conditioning studies. On the whole, we believe they are in fair accord with our theoretical schema, including

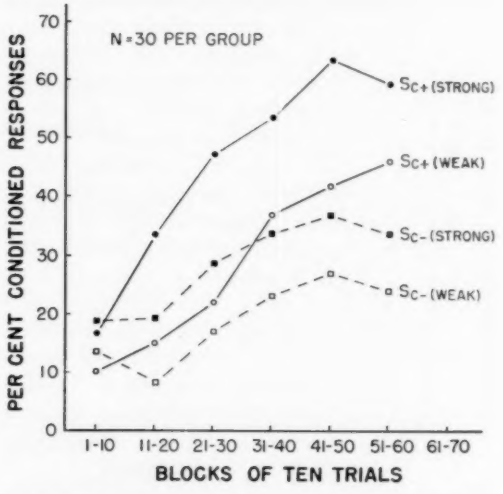

FIG. 6. Percentage of CR's to the positive and negative stimuli for groups conditioned with a strong and weak US.

the portion of it that involves the A-scale. While not all of the results have met acceptable levels of significance, the fact that the direction of the differences in such instances has almost invariably been in accord with the theory has encouraged us to continue to hold to it. Attention might also be called here to the fact that this theoretical model, particularly the hypothetical emotional response mechanism, has also been quite successful in connection with a wide variety of other behavioral situations involving noxious stimulation with animals. Examples are to be found in the many studies cited by Miller (13) on the motivating and reinforcing roles of acquired fear in learning situations. The experiments of a number of investigators on the persisting motivational effects of emotionality aroused by electric shock on the consummatory behavior of rats provide yet another example (1, 2, 18, 19).

Complex Human Learning

Turning now to our studies involving the more complex types of human learning, let me begin by saying that it is in this area that the limitations of space in experimental journals for theoretical elaboration have been most unfortunate. Certainly we recognize that these treatments have been quite inadequate, particularly from the point of view of discussing the many factors that complicate efforts at theorizing in this area. By way of example let me mention two important points that need to be recognized but, unfortunately, have not always been so.

First, it should be realized that in order to derive implications concerning the effects of drive variation in any type of complex learning task, it is necessary to have, in addition to the drive theory, a further theoretical network concerning the variables and their interaction that are involved in the particular learning activity. It is perhaps unnecessary to point out here that theoretical schemas for such types of learning are as yet in a very primitive state of development, indeed almost nonexistent. As a consequence of this, one has considerable difficulty in drawing conclusions about the motivational part of the new, combined theory from supposedly negative findings, for the defect may be in the part of the network specifying the action of the variables in the complex learning situation.

The second point is that our theory of the mechanism underlying *D* was developed in connection with experimental situations involving some form of noxious stimulation. Complex human learning tasks, on the other hand, typically do not involve the use of a noxious stimulus. Whatever stress is present in these situations is usually produced by instructions that aim to create in the subject the desire or need to make as good a showing as possible. While it is true that this stress may be greatly augmented by introducing failure or punishment into the situation, so far as the usual type of human learning experiment is concerned, the question as to whether High A subjects would be more emotional than Low A subjects, and hence have a higher *D* level, is a moot one. In this connection two alternative subhypotheses have been proposed: (*a*) the chronic hypothesis: that High A subjects react emotionally in a chronic manner to all situations, whether stressful or not; and (*b*) the emotional reactivity hypothesis: that High A subjects have a lower threshold of emotional responsiveness and react with a stronger emotional response than Low A subjects to situations containing some degree of stress (16, 20, 25). As may be seen, according to the first of these hypotheses, mild nonthreatening situations would produce a differential drive (*D*) level in subjects scoring at extremes of the scale; whereas according to the second, there would not be a difference. These two examples are sufficient, I believe, to point up the fact that the problems involved in the extension of the theory to these more complex types of learning are quite formidable and that at this stage there necessarily must be a considerable amount of trial and error in our theorizing.

Now it will be recalled that the theoretical schema presented in Fig. 1 assumed that in classical conditioning habit strength to but a single response was established to the CS. In this circumstance, as we have seen, an increase in drive level implied an increase in response strength. In more complex, selective learning tasks, on the other hand, there are, typically, a hierarchy of competing response tendencies. Actually most of the complex learning situations employed with humans involve a number or sequence of such response hierarchies which involve competing responses, e.g., a number of choice points in the maze, whether verbal or spatial. To show what the implications of variation of drive level will be in such competing response situations, let us begin by considering the simplest conceivable case: one in which there is but

a single response hierarchy involving two alternative responses. The single choice point maze involving turning left or right is one example of such a situation. If now the habit strength of the correct to-be-learned response is, at the beginning of the learning, somewhat stronger than that of the incorrect response, it may be shown that the higher the drive level, D, the greater will the difference between the competing excitatory potentials be and, *neglecting all other considerations*, the higher should be the percentage of correct responses at the start of learning, the sooner should the learning criterion be attained, and the smaller should be the total number of errors.[3]

The reverse situation, that in which the correct response is at the outset weaker than the incorrect one, is, from the theoretical viewpoint, even more complex. In this instance the stronger the drive, the greater will be the percent choice of the wrong response, or, in other words, the poorer will be the performance at this initial stage. But, as training proceeds, sooner or later the habit strength of the correct, reinforced response will overtake that of the wrong, unreinforced response and from this point on the percent choice of the correct response should in general be higher for the high drive group than for the low drive group. In other words, the performance curves should be expected to cross. Precise predictions about the total number of errors, number of trials, etc. in this situation will depend to a considerable extent upon the particular functions and parameter values assigned to the assumed habit and inhibitory factors. Actually we have never got around to working out in detail the implications of the various possibilities for the total learning period even in this simplest case.

Recalling now that such a learning task as the serial verbal or spatial maze involves a number of such competing response hierarchies, we see that the problem of predicting the effect on performance of variation of the drive in such situations becomes even more complicated. On the assumption that anticipatory and perseverative associative tendencies would develop in such a manner as to make the incorrect response the stronger in the case of many of the choice points of a maze, it was hoped that it would be possible to demonstrate that high drive (i.e., High A) subjects would perform more poorly in such serial learning situations than low drive (i.e., Low A) subjects. Two experiments, one with a verbal form of maze (35) and one using a finger maze (5) actually did provide results in agreement with this theoretical expectation. However, as was pointed out at the time, there was a serious discrepancy between the theory and the obtained results in these studies in that the anxious subjects made more errors at all but one of the choice points in both studies. In view of the ease of learning many of these choice points, and hence evidence for little or no strong competing response tendencies, the theory would have led us to expect that the High A subjects would have made fewer errors on them than the Low A subjects. Obviously the theory was wrong in some respect, but just in what way—an incorrect assumption or failure to include an important relevant variable—was not clear.

At this point in our work we realized that such serial learning tasks are, for a variety of reasons, quite unsatisfactory. Among the most important from our viewpoint was the fact that one has little or no knowledge of the relative strengths of the competing responses in each of the hierarchies. Accordingly we abandoned this type of situation and attempted to develop learning tasks in which it would be possible to specify or manipulate in some known manner the relative strengths of the competing responses in each hierarchy. Probably the chief value of these earlier experiments is that they did point up the fact that a higher anxiety score (and hence possibly a higher drive level) does not necessarily always lead to a higher level of performance.

[3] As discussed in my Silliman Lectures (21), there are a number of other considerations that need to be taken into account in extending the theory to such competing response situations. Thus the particular composition rule (law) assumed in these lectures to describe the manner in which the competing responses interacted with each other led to the implication that the percentage of occurrence of the competing responses is a function, not only of the magnitude of the difference between the competing Es, but also of their absolute level above the threshold L. As a consequence in the low range of E values, there may actually be an inverse relation between performance level (percent choice of the response with stronger E) and the level of drive. Still other considerations involve whether habit strength (H) in learning situations is or is not assumed to be dependent on the reinforcer and whether drive strength (D) determines the inhibitory factor (I_s). Different combinations of these alternative assumptions, including even other possible composition rules, lead to different behavior consequences. Critical evaluation of the different conceivable theoretical models will require considerably more empirical data obtained under a wide variety of experimental conditions than is now available.

Among the types of learning problems that we turned to was paired-associates learning. This type of learning task may be conceived as consisting of the formation of a set of more or less isolated S-R associations or habit tendencies. In one type of list, which we have referred to as a noncompetitive list, an attempt is made to isolate as much as possible the paired items by minimizing the degree of synonymity or formal similarity among both the stimulus and response words. As learning proceeds and the habit strengths of the stimulus words to their paired response words increase, high drive subjects should, according to our theory, perform at a higher level than low drive subjects. An important condition in this derivation is that the associative connections between each stimulus word and the nonpaired response words are lower than that to the paired response word.

Two lists of this type have been employed. In one the associative connections between the paired words were initially zero or at least very low. In this type of list it would be predicted that there would be little or no difference between high and low drive subjects at the start of learning, but that as learning progressed the curve of correct responses would diverge, that for the high drive group being the higher. Using nonsense syllables of low association value and low intralist similarity, Taylor has reported two experiments in which this type of list was employed (33, 34). The lower pair of curves in Fig. 7 present the data from one of these studies (34). Both curves, it will be observed, began at a very low level with the curve for the High A group (solid line) rising above that for the Low A group (broken line). An unpublished study from our laboratory employing nonassociated paired adjectives has given similar results, although the superiority of the High A over the Low A subjects was significant only on a single tailed hypothesis.

The second type of noncompetitive list differs from the first in that the associative strengths of the paired words are, as the result of past experiences, considerably above zero. Under this condition it would be predicted that the performance curves would, on the first anticipation trial, be considerably above 0% and that the curve for the high drive group would be above that for the low drive group. Employing paired adjectives that had been scaled by Haagen (7) as having high "closeness of association" values, two studies (24, 29) have reported results which support these implications.

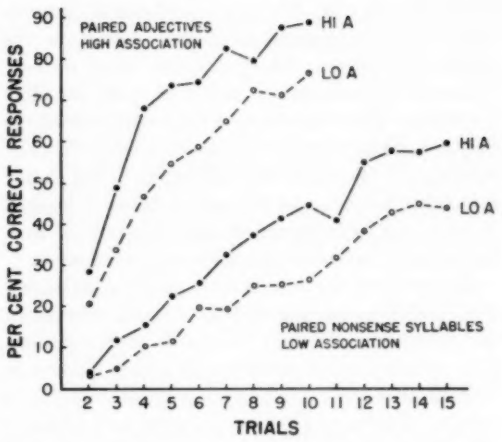

FIG. 7. Paired-associates learning as a function of A-score under conditions of unusual interword pair competition.

The upper pair of curves in Fig. 7 shows the findings of one of these studies (29). As may be seen, the initial level of performance was well above 0 and the High A subjects started out and continued at a higher level than the Low A subjects. On the other hand, a recently completed doctoral dissertation (6) using this type of list failed to obtain results in accord with the theory. There was little or no difference between the two groups at any stage of practice.

In contrast to these noncompetitive type lists we have also designed a competitive list which includes some paired items in which the initial habit strength of the stimulus word to call out the paired word is weaker than the habit strengths to one or more other response words in the list. In the case of these items it would be predicted from our theory that high drive subjects would at the start of learning perform more poorly than low drive subjects. Here again we should have emphasized that the theory of paired-associates learning has as yet not been developed sufficiently to predict what will happen beyond the first few trials, and it would have been more appropriate, as far as implications for our drive theory are concerned, if we had used at most only the data from the first four or five trials. Precise predictions concerning performance beyond this point must await the development of a more adequate theory of the variables determining the weakening of these stronger, incorrect responses in paired-associates learning. Two pub-

lished studies (24, 29) and one doctoral dissertation (6) have reported data with respect to the implication of our theory for this type of list; while all three found, as predicted, that the High A subjects were inferior to Low A subjects in the first four trials, none of the results was statistically significant. However, the implication of the theory that there would be an interaction between level of A-score and performance on the two kinds of lists, competitive and noncompetitive, was confirmed.

Summarizing the results with these paired-associates lists, I would say that the batting average of our theory is fairly high but by no means perfect. It is clearly evident from the data that differences in level of A-score (and hence level of D), if it is a factor determining performance on such tasks, is a relatively unimportant one. Certainly individual differences in verbal learning ability play a much more decisive role. Moreover, there are as yet many factors that play important roles in such complex behavior situations, about which we have as yet little or no knowledge. Among those of a motivational nature is the type of task-irrelevant response that Child and his group have studied (3). We think of these interfering responses as being elicited by drive stimuli (s_D), and hence they would be incorporated in a more complete motivational theory of learned behavior. On the basis of evidence in the literature and some recently completed studies of our own, we believe this factor is especially important when shock is introduced into verbal learning situations.

I should like to conclude this presentation by stating very briefly the purpose of such theoretical schemas as has been presented here. As I conceive them, their primary function is to provide for the unification of what, without the theory, would be a multiplicity of isolated or unconnected facts and laws. Thus, in the present instance, such phenotypically different phenomena as behavior in eyelid conditioning under various stimulus conditions, degree of emotionality as revealed by a personality questionnaire and physiological measures, and such opposite performance differentials in paired-associates tasks as just described have been interrelated by means of the theory. That much work, both of a theoretical and experimental nature, remains to be done in this area of behavior study is clearly revealed by the many gaps and deficiencies in the present attempt. It is my firm belief, however, that progress in the development of this, as in any other scientific field of knowledge, is greatly facilitated by such theoretically oriented research endeavors.

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A PROCESS CONCEPTION OF PSYCHOTHERAPY

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I WOULD like to take you with me on a journey of exploration. The object of the trip, the goal of the search, is to try to learn something of the *process* of psychotherapy, or the *process* by which personality change takes place. I would warn you that the goal has not yet been achieved and that it seems as though the expedition has advanced only a few short miles into the jungle. Yet perhaps if I can take you with me, you will be tempted to discover new and profitable avenues of further advance.

THE PUZZLE OF PROCESS

My own reason for engaging in such a search seems simple to me. Just as many psychologists have been interested in the invariant aspects of personality—the unchanging aspects of intelligence, temperament, personality structure—so I have long been interested in the invariant aspects of *change* in personality. Do personality and behavior change? What commonalities exist in such changes? What commonalities exist in the conditions which precede change? Most important of all, what is the process by which such change occurs?

Puzzling over this problem of getting at the process has led me to realize how little objective research deals with process in any field. Objective research slices through the frozen moment to provide us with an exact picture of the interrelationships which exist at that moment. But our understanding of the ongoing movement—whether it be the process of fermentation, or the circulation of the blood, or the process of atomic fission—is generally provided by a theoretical formulation, often supplemented, where feasible, with a clinical observation of the process. I have thus come to realize that perhaps I am hoping for too much to expect that research procedures can shed light directly upon the process of personality change. Perhaps only theory can do that.

A Rejected Method

When I determined, more than a year ago, to make a fresh attempt to understand the manner in which such change takes place, I first considered various ways in which the experience of therapy might be described in terms of some other theoretical framework. There was much that was appealing in the field of communication theory, with its concepts of feedback, input and output signals, and the like. There was the possibility of describing the process of therapy in terms of learning theory or in terms of general systems theory. As I studied these avenues of understanding, I became convinced that it would be possible to translate the process of psychotherapy into any one of these theoretical frameworks. It would, I believe, have certain advantages to do so. But I also became convinced that, in a field so new, this is not what is most needed.

I came to a conclusion which others have reached before: in a new field perhaps what is needed first is to steep oneself in the *events,* to approach the phenomena with as few preconceptions as possible, to take a naturalist's observational, descriptive approach to these events, and to draw forth those low-level inferences which seem most native to the material itself.

The Mode of Approach

So, for the past year, I have used the method which so many of us use for generating hypotheses, a method which psychologists in this country seem so reluctant to expose or comment on. I used myself as a tool. I have spent many hours listening to recorded therapeutic interviews—trying to listen as naively as possible. I have endeavored to soak up all the clues I could capture as to the process, as to what elements are significant in change. Then I have tried to abstract from that sensing the simplest abstractions which would describe them. Here I have been much stimulated and helped by the thinking of many of my colleagues, but I would like to mention my special indebtedness to Eugene Gendlin, William Kirtner, and Fred Zimring, whose demonstrated ability to think in new ways about these matters has been particularly helpful and from whom I have borrowed heavily.

The next step has been to take these observations and low-level abstractions and formulate them in such a way that testable hypotheses can readily be drawn from them. This is the point I have reached. I make no apology for the fact that I am reporting no empirical investigations of these formulations. If past experience is any guide, then I may rest assured that, if the formulations I am about to present check in any way with the subjective experience of other therapists, then a great deal of research will be stimulated, and in a few years there will be ample evidence of the degree of truth and falsity in the statements which follow.

A Basic Condition

If we were studying the process of growth in plants, we would assume certain constant conditions of temperature, moisture, and sunlight in forming our conceptualizations of the process. Likewise in conceptualizing the process of personality change in psychotherapy, I shall assume a constant and optimal set of conditions for facilitating this change. I have recently tried to spell out these conditions in some detail (**7**). For our present purpose, I believe I can state this assumed condition in one word. Throughout the discussion which follows, I shall assume that the client experiences himself as being fully *received*. By this I mean that, whatever his feelings—fear, despair, insecurity, anger; whatever his mode of expression—silence, gestures, tears, or words; whatever he finds himself being in this moment, he senses that he is psychologically *received,* just as he is, by the therapist. There is implied in this term the concept of being understood, empathically, and the concept of acceptance. It is also well to point out that it is the client's experience of this condition which makes it optimal, not merely the fact of its existence in the therapist.

In all that I shall say, then, about the process of change, I shall assume as a constant an optimal and maximum condition of being received.

The Emerging Continuum

In trying to grasp and conceptualize the process of change, I was initially looking for elements which would mark or characterize change itself. I was thinking of change as an entity and searching for its specific attributes. What gradually emerged in my understanding as I exposed myself to the raw material of change was a continuum of a different sort than I had conceptualized before.

Individuals move, I began to see, not from a fixity or homeostasis through change to a new fixity, though such a process is indeed possible. But much the more significant continuum is from fixity to changingness, from rigid structure to flow, from stasis to process. I formed the tentative hypothesis that perhaps the qualities of the client's expression at any one point might indicate his position on this continuum, might indicate where he stood in the process of change.

SEVEN STAGES OF PROCESS

I gradually developed this concept of a continuum of process, discriminating seven stages in it, with examples from recorded therapeutic interviews illustrating the qualities of the process at each stage. It would be quite impossible to give all of this crude scale here, but I shall try to suggest something of its nature by describing very briefly Stages 1 and 2, to illustrate the lower end of the continuum, and describing more fully Stages 5, 6, and 7, to fill in the upper end of the scale.[1]

First Stage

The individual in this stage of fixity and remoteness of experiencing is not likely to come voluntarily for therapy. However, I can to some degree describe the characteristics of this stage:

There is an unwillingness to communicate self. Communication is only about externals.
Feelings and personal meanings are neither recognized as such nor owned.
Personal constructs (to use Kelly's helpful term, **4**) *are extremely rigid.*
Close and communicative relationships are construed as dangerous.
No problems are recognized or perceived at this stage.
There is no desire to change.
There is much blockage of internal communication.

Perhaps these brief statements will convey something of the psychological fixity of this end of the continuum. The individual has little or no recogni-

[1] An amplification of this paper, giving the whole scale with more extended illustrations, may be obtained from the author by those who are interested in using it for research purposes.

tion of the ebb and flow of the feeling life within him. He construes his experience rigidly in terms of the past. He is (to borrow the term of Gendlin and Zimring) structure-bound in his manner of experiencing, reacting to now "by finding it to be like a past experience and then reacting to that past, feeling *it*" (3). The individual at this stage represents stasis, fixity, the opposite of flow or change.

Second Stage of Process

When the person in the first stage can experience himself as fully received, then the second stage follows. We seem to know very little about how to provide the experience of being received for the person in the first stage, but it is occasionally achieved in play or group therapy where the person can be exposed to a receiving climate, without himself having to take any initiative, for a long enough time to experience himself *as received*. In any event where he does experience this, then a slight loosening and flowing of symbolic expression occurs, which tends to be characterized by the following:

Expression begins to flow in regard to nonself topics.

Ex. "I guess that I suspect my father has often felt very insecure in his business relations."[2]

Problems are perceived as external to self.

Ex. "Disorganization keeps cropping up in my life."

There is no sense of personal responsibility in problems.

Ex. This is illustrated in the above excerpt.

Feelings are described as unowned, or sometimes as past objects.

Ex. Counselor: "If you want to tell me something of what brought you here. . . ." Client: "The symptom was —it was—just being very depressed." This is an excellent example of the way in which internal problems can be perceived and communicated about as entirely external. She is not saying "I am depressed" or even "I was depressed." Her feeling is handled as a remote, unowned object, entirely external to self.

Feelings may be exhibited, but are not recognized as such or owned.

Experiencing is bound by the structure of the past.

Ex. "I suppose the compensation I always make is, rather than trying to communicate with people or have the right relationship with them, to compensate by, well, shall we say, being on an intellectual level." Here the client is beginning to recognize the way in which her experiencing is bound by the past. Her statement also illustrates the remoteness of experiencing at this level. It is as though she were holding her experience at arm's length.

Personal constructs are rigid, and unrecognized as being constructs, but are thought of as facts.

Ex. "I can't ever do anything right—can't ever finish it."

Differentiation of personal meanings and feelings is very limited and global.

The preceding example is a good illustration. "I can't *ever*" is one instance of a black and white differentiation, as is also the use of "right" in this absolute sense.

Contradictions may be expressed, but with little recognition of them as contradictions.

Ex. "I want to know things, but I look at the same page for an hour."

As a comment on this second stage of the process of change, it might be said that a number of clients who voluntarily come for help are in this stage, but we (and probably therapists in general) have a very minimal degree of success in working with them. This seems, at least, to be a reasonable conclusion from Kirtner's study (5), though his conceptual framework was somewhat different. We seem to know too little about the ways in which a person at this stage may come to experience himself as "received."

The Fifth Stage

I shall omit any description of Stages 3 and 4. Each involves a further loosening of symbolic expression in regard to feelings, constructs, and self. These stages constitute much of psychotherapy. But going beyond these stages, we can again mark a point on the continuum and call it Stage 5. If the client feels himself received in his expressions, behaviors, and experiences at the third and fourth stage, then this sets in motion still further loosenings, and the freedom of organismic flow is increased. Here I believe we can again delineate crudely the qualities of this phase of the process:

Feelings are expressed freely as in the present.

Ex. "I expected kinda to get a severe rejection—this I expect all the time . . . somehow I guess I even feel it with you. . . . It's hard to talk about because I want to be the best I can possibly be with you." Here feelings regarding the therapist and the client in relationship to the therapist, emotions often most difficult to reveal, are expressed openly.

[2] The many examples used as illustrations are taken from recorded interviews, unless otherwise noted. For the most part, they are taken from interviews which have never been published, but a number of them are taken from the report of two cases in a chapter of a forthcoming book (6).

Feelings are very close to being fully experienced. They "bubble up," "seep through," in spite of the fear and distrust which the client feels at experiencing them with fullness and immediacy.

Ex. Client is talking about an external event. Suddenly she gets a pained, stricken look. Therapist: "What—what's hitting you now?" Client: "I don't know. (She cries) . . . I must have been getting a little too close to something I didn't want to talk about." The feeling has almost seeped through into awareness, in spite of her.

There is a beginning tendency to realize that experiencing a feeling involves a direct referent.

The example just cited illustrates this in part. The client knows she has experienced something, knows she is not clear as to what she has experienced. But there is also the dawning realization that the referent of these vague cognitions lies within her, in an organismic event against which she can check her symbolization and her cognitive formulations. This is often shown by expressions that indicate the closeness or distance the individual feels from this referent. Ex. "I really don't have my finger on it. I'm just kinda describing it."

There is surprise and fright, rarely pleasure, at the feelings which "bubble through."

Ex. Client, talking about past home relationships, "That's not important any more. Hmm. [Pause] That was somehow very meaningful—but I don't have the slightest idea why. . . . Yes, that's it! I can forget about it now and—why, it *isn't* that important. Wow! All that miserableness and stuff!"

There is an increasing ownership of self feelings, and a desire to be these, to be the "real me."

Ex. "The real truth of the matter is that I'm not the sweet, forbearing guy that I try to make out that I am. I get irritated at things. I feel like snapping at people, and I feel like being selfish at times; and I don't know why I should pretend I'm *not* that way." This is a clear instance of the greater degree of acceptance of all feelings

Experiencing is loosened, no longer remote, and frequently occurs with little postponement.

There is little delay between the organismic event and the full subjective living of it. A beautifully precise account of this is given by a client. Ex. "I'm still having a little trouble trying to figure out what this sadness—and the weepiness—means. I just know I feel it when I get close to a certain kind of feeling—and usually when I do get weepy, it helps me to kinda break through a wall I've set up because of things that have happened. I feel hurt about something and then automatically this kind of shields things up and then I feel like I can't really touch or feel *anything* very much . . . and if I'd be *able* to feel, or could *let* myself feel the instantaneous feeling when I'm hurt, I'd immediately start being weepy right then, but I can't."

Here we see him regarding his feeling as an inner referent to which he can turn for greater clarity. As he senses his weepiness, he realizes that it is a delayed and partial experiencing of being hurt. He also recognizes that his defenses are such that he can not, at this point, experience the event of hurt when it occurs.

The ways in which experience is construed are much loosened. There are many fresh discoveries of personal constructs as constructs, and a critical examination and questioning of these.

Ex. A man says: "This idea of needing to please people —of *having* to do it—that's really been kind of a basic assumption of my life (he weeps quietly). It's kind of, you know, just one of the very unquestioned axioms that I *have* to please. I have no choice. I just *have* to." Here he is clear that this assumption has been a construct, and it is evident that its unquestioned status is at an end.

There is a strong and evident tendency toward exactness in differentiation of feelings and meanings.

A client speaks of "Some tension that grows in me, or some hopelessness, or some kind of incompleteness—and my life actually is very incomplete right now. . . . I just don't know. Seems to be, the closest thing it gets to, is *hopelessness.*" Obviously he is trying to capture the exact term which for him symbolizes his experience.

There is an increasingly clear facing of contradictions and incongruences in experience.

Ex. "My conscious mind tells me I'm worthy. But someplace inside I don't believe it. I think I'm a rat—a no-good. I've no faith in my ability to do anything."

There is an increasing quality of acceptance of self-responsibility for the problems being faced, and a concern as to how he has contributed.

There are increasingly freer dialogues within the self—an improvement in, *and reduced blockage* of, *internal communication.*

Sometimes these dialogues are verbalized. Ex. "Something in me is saying: 'What more do I have to give up? You've taken so much from me already.' This is *me* talking to *me*—the *me* way back in there who talks to the *me* who runs the show. It's complaining now, saying, 'You're getting too close! Go away!' "

I trust that the examples I have given of this fifth phase of the process continuum will make several points clear. In the first place, this phase is several hundred psychological miles from the first stage described. Here many aspects of the client are in flow, as against the rigidity of the first stage. He is very much closer to his organic being, which is always in process. He is much closer to being in the flow of his feelings. His constructions of experience are decidedly loosened and repeatedly being tested against referents and evidence within and without. Experience is much more highly differentiated, and thus internal communication, already flowing, can be much more exact.

As a general comment on the description thus far, it would be my observation that a person is never wholly at one or another stage of the process. There is, however, a general consistency in his manner of experiencing and expressing. Thus, a client who is generally at Stage 2 or 3 seems unlikely to exhibit any behaviors characteristic of Stage 5. This is especially true if we limit observations to a single defined area of related personal meanings in the client. Then I would hypothesize that there will be considerable regularity, that Stage 3 would rarely be found before Stage 2, that Stage 4 would rarely follow Stage 2 without Stage 3 intervening. Such tentative hypotheses can, of course, be put to empirical test.

The Sixth Stage

If I have been able to communicate some feeling for the scope and quality of the increased loosening, at each stage, of feeling, experiencing, and construing, then we are ready to look at the next stage, which appears, from observation, to be a very crucial one. Let me see if I can convey what I perceive to be its characteristic qualities.

Assuming that the client continues to be fully received in the therapeutic relationship, then the characteristics of Stage 5 tend to be followed by a very distinctive and often dramatic phase. It is characterized as follows:

A feeling which has previously been "stuck," has been inhibited in its process quality, is experienced with immediacy now.

A feeling flows to its full result.

A present feeling is directly experienced with immediacy and richness.

This immediacy of experiencing, and the feeling which constitutes its content, are accepted. This is something which is, *not something to be denied, feared, struggled against.*

All the preceding sentences attempt to describe slightly different facets of what is, when it occurs, a clear and definite phenomenon. It would take recorded examples to communicate its full quality, but I shall try to give an illustration without benefit of recording. A somewhat extended excerpt from the eightieth interview with a young man may communicate the way in which a client comes into Stage 6.

Client: "I could even conceive of it as a possibility that I could have a kind of tender concern for me. . . . Still, how could *I* be tender, be concerned for *myself,* when they're one and the same thing? But yet I can *feel* it so clearly. . . . You know, like taking care of a child. You want to give it this and give it that. . . . I can kind of clearly see the purposes for somebody else . . . but I can never see them for . . . myself, that I could do this for me, you know. Is it possible that I can really want to take care of myself, and make that a major purpose of my life? That means I'd have to deal with the whole world as if I were guardian of the most cherished and most wanted possession, that this *I* was between this precious *me* that I wanted to take care of and the whole world. . . . It's almost as if I *loved* myself—you know—that's strange—but it's true." Therapist: "It seems such a strange concept to realize. Why, it would mean I would face the world as though a part of my primary responsibility was taking care of this precious individual who is me—whom I love." Client: "Whom I care for—whom I feel so *close* to. Woof!! That's another *strange* one." Therapist: "It just seems *weird.*" Client: "Yeah. It hits rather close somehow. The idea of my loving me and the taking care of me. [His eyes grow moist] That's a very, very nice one—very nice."

The recording would help to convey the fact that here is a feeling which had never been able to flow in him, which is experienced with immediacy, in this moment. It is a feeling which flows to its full result, without inhibition. It is experienced acceptantly, with no attempt to push it to one side or to deny it.

There is a quality of living subjectively in *the experience, not feeling* about *it.*

The client, in his *words,* may withdraw enough from the experience to feel about it, as in the above example, yet the recording makes it clear that his words are peripheral to the experiencing which is going on within him and in which he is living. The best communication of this in his words is "Woof!! That's another *strange* one."

Self as an object tends to disappear.

The self, at this moment, *is* this feeling. This is a being in the moment, with little self-conscious awareness, but with primarily a reflexive awareness, as Sartre terms it. The self *is,* subjectively, in the existential moment. It is not something one perceives.

Experiencing, at this stage, takes on a real process quality.

One client, a man who is approaching this stage, says that he has a frightened feeling about the source of a lot of secret thoughts in himself. He goes on: "The butterflies are the thoughts closest to the surface. Underneath there's a deeper flow. I feel very removed from it all. The deeper flow is like a great school of fish moving under the surface. I see the ones that break through the surface of the water —sitting with my fishing line in one hand, with a bent pin on the end of it—trying to find a better tackle—or better yet, a way of diving in. That's the scary thing. The image I get is that *I* want to be one of the fish myself." Therapist: "You want to be down there flowing along, too."

Though this client is not yet fully experiencing in a process manner, and hence does not fully exemplify this sixth point on the continuum, he foresees it so clearly that his description gives a real sense of its meaning.

Another characteristic of this stage of process is the physiological loosening which accompanies it.

Moistness in the eyes, tears, sighs, muscular relaxation are frequently evident. Often there are other physiological concomitants. I would hypothesize that in these moments, had we the measures for it, we would discover improved circulation, improved conductivity of nervous impulses. An example of the "primitive" nature of some of these sensations may be indicated in the following excerpt:

The client, a young man, has expressed the wish his parents would die or disappear. "It's kind of like wanting to wish them away, and wishing they had never been. . . . And I'm so ashamed of myself because then they call me, and off I go—swish! They're somehow still so strong. I don't know. There's some umbilical—I can almost feel it inside me—swish" [and he gestures, plucking himself away by grasping at his navel]. Therapist: "They really do have a hold on your umbilical cord." Client: "It's funny how real it feels . . . like a burning sensation, kind of, and when they say something which makes me anxious I can feel it right here [pointing]. I never thought of it quite that way." Therapist: "As though, if there's a disturbance in the relationship between you, then you do just feel it as though it was a strain on your umbilicus." Client: "Yeah, kind of like in my gut here It's so hard to define the feeling that I feel there."

Here he is living subjectively in the feeling of dependence on his parents. Yet it would be inaccurate to say that he is perceiving it. He is *in* it, experiencing it as a strain on his umbilical cord.

In this stage, internal communication is free and relatively unblocked.

I believe this is quite adequately illustrated in the examples given. Indeed the phrase "internal communication" is no longer quite correct; for, as each of these examples illustrates, the crucial moment is a moment of integration, in which communication between different internal foci is no longer necessary, because they become *one*.

The incongruence between experience and awareness is vividly experienced as it disappears into congruence.

The relevant personal construct is dissolved in this experiencing moment, and the client feels cut loose from his previously stabilized framework.

I trust these two characteristics may acquire more meaning from the following example. A young man has been having difficulty getting close to a certain unknown feeling. "That's almost exactly what the feeling is, too—it was that I was living so much of my life, and seeing so much of my life, in terms of being *scared* of something." He tells how his professional activities are just to give him a little safety and "a little world where I'll be secure, you know. And for the same reason. [Pause] I was kind of letting it seep through. But I also tied it in with you and with my relationship with you, and one thing I feel about it is fear of its going away. [His tone changes to role-play more accurately his feeling.] Won't you let me have this? I kind of *need* it. I can be so lonely and scared without it." Therapist: "M-hmm, m-hmm. 'Let me hang on to it because I'd be terribly scared if I didn't!' . . . It's a kind of pleading thing too, isn't it?" Client: "I get a sense of —it's this kind of pleading little boy. It's this gesture of begging" [putting his hands up as if in prayer]. Therapist: "You put your hands in kind of a supplication." Client: "Yeah, that's right. *'Won't* you do this for me?' kind of. Oh, that's terrible! Who, *me? Beg?* . . . That's an emotion I've never felt clearly at all—something I've never been. . . . [Pause] . . . I've got such a confusing feeling. One is, it's such a wondrous feeling to have these new things come out of me. It amazes me so much each time, and there's that same feeling, being scared that I've so much of this. [Tears] . . . I just don't know myself. Here's suddenly something I never realized, hadn't any inkling of—that it was some *thing* or some *way* I wanted to be."

Here we see a complete experiencing of his pleadingness, and a vivid recognition of the discrepancy between this experiencing and his concept of himself. Yet this experiencing of discrepancy exists in the moment of its disappearance. From now on he *is* a person who feels *pleading*, as well as many other feelings. As this moment dissolves the way he has construed himself, he feels cut loose from his previous world—a sensation which is both wondrous and frightening.

The moment of full experiencing becomes a clear and definite referent.

The examples given should indicate that the client is often not too clearly aware of what has "hit him" in these moments. Yet this does not seem too important because the event is an entity, a referent, which can be returned to, again and again if necessary, to discover more about it. The pleadingness, the feeling of "loving myself" which are present in these examples, may not prove to be exactly as described. They are, however, solid points of reference to which the client can return until he has satisfied himself as to what they are. It is, perhaps, that they constitute a clear-cut physiological event, a substratum of the conscious life, which the client can return to for investigatory purposes. Gendlin has called my attention to this significant quality of experiencing as a referent. He is endeavoring to build an extension of psychological theory on this basis (2, especially Chap. 7).

Differentiation of experiencing is sharp and basic.

Because each of these moments is a referent, a specific entity, it does not become confused with anything else. The process of sharp differentiation builds on it and about it.

In this stage there are no longer "problems," external or internal. The client is living, *subjectively, a phase of his problem. It is not an object.*

I trust it is evident that in any of these examples it would be grossly inaccurate to say that the client perceives his problem as internal or is dealing with it as an internal problem. We need some way of indicating that he is further than this and, of course, enormously far in the

process sense from perceiving his problem as external. The best description seems to be that he neither perceives his problem nor deals with it. He is simply living some portion of it knowingly and acceptingly.

I have dwelt so long on this sixth definable point on the process continuum because I see it as a highly crucial one. My observation is that these moments of immediate, full, accepted experiencing are in some sense almost irreversible. To put this in terms of the examples, it is my observation and hypothesis that with these clients, whenever a future experiencing of the same quality and characteristics occurs, it will necessarily be recognized in awareness for what it is: a tender caring for self, an umbilical bond which makes him a part of his parents, or a pleading small-boy dependence, as the case may be. And, it might be remarked in passing, once an experience is fully in awareness, fully accepted, then it can be coped with effectively, like any other clear reality.

The Seventh Stage

In those areas in which the sixth stage has been reached, it is no longer so necessary that the client be fully received by the therapist, though this still seems helpful. However, because of the tendency for the sixth stage to be irreversible, the client often seems to go on into the seventh and final stage without much need of the therapist's help. This stage occurs as much outside of the therapeutic relationship as in it and is often reported, rather than experienced, in the therapeutic hour. I shall try to describe some of its characteristics as I feel I have observed them:

New feelings are experienced with immediacy and richness of detail, both in the therapeutic relationship and outside.

The experiencing of such feelings is consciously used as a clear referent.

There is a growing and continuing sense of acceptant ownership of these changing feelings, a basic trust in his own process.

This trust is not primarily in the conscious processes which go on, but rather in the total organismic process. One client puts it: "I seem to work best when my conscious mind is only concerned with facts and letting the analysis of them go on by itself without paying any attention to it."

Experiencing has lost almost completely its structure-bound aspects and becomes process experiencing—that is, the situation is experienced and interpreted in its newness, not as the past.

An example in a very specific area is given by a client in a follow-up interview as he explains the different quality that has come about in his creative work. It used to be that he tried to be orderly. "You begin at the beginning and you progress regularly through to the end." Now he is aware that the process in himself is different. "When I'm working on an idea, the whole idea develops like the latent image coming out when you develop a photograph. It doesn't start at one edge and fill in over to the other. It comes in *all over*. At first all you see is the hazy outline, and you wonder what it's going to be; and then gradually something fits here and something fits there, and pretty soon it all comes clear—all at once." It is obvious that he has not only come to trust this process, but that he is experiencing it as it *is*, not in terms of some past.

The self becomes increasingly simply the subjective and reflexive awareness of experiencing. The self is much less frequently a perceived object and much more frequently something confidently felt in process.

An example may be taken from the same follow-up interview with the client quoted above. In this interview, because he is reporting his experience since therapy, he again becomes aware of himself as an object; but it is clear that this has not been the quality of his day-by-day experience. After reporting many changes, he says: "I hadn't really thought of any of these things in connection with therapy until tonight. . . . [Jokingly] Gee! maybe something *did* happen. Because my life since *has* been different. My productivity has gone up. My confidence has gone up. I've become brash in situations I would have avoided before. And also, I've become less brash in situations where I would have become very obnoxious before." It is clear that only afterward does he realize what his self as an object has been.

Personal constructs are tentatively reformulated, to be validated against further experience, but even then to be held loosely.

A client describes the way in which such a construct changed, between interviews, toward the end of therapy: "I don't know what [changed], but I definitely feel different about looking back at my childhood, and some of the hostility about my mother and father has evaporated. I substituted for a feeling of resentment about them a sort of acceptance of the fact that they did a number of things that were undesirable with me. But I substituted a sort of feeling of interested excitement that—gee—now that I'm finding out what was wrong, *I* can do something about it—correct their mistakes." Here the way in which he construes his experience with his parents has been sharply altered.

Internal communication is clear, with feelings and symbols well matched, and fresh terms for new feelings.

There is the experiencing of effective choice of new ways of being.

Because all the elements of experience are available to awareness, choice becomes real and effective. Here a client is just coming to this realization: "I'm trying to encompass a way of talking that is a way out of being scared of talking. Perhaps just kind of thinking out loud is the way to do that. But I've got so *many* thoughts I could only do it a little bit. But maybe I could let my talk be an expression of my thoughts, instead of just trying to make the proper noises in each situation." Here he is sensing the possibility of effective choice, perhaps approaching this seventh stage rather than being in it.

By no means all clients move this far on the continuum; but when this seventh stage is reached, it involves us in another dimension. For it will be evident that the client has now incorporated the quality of motion, of flow, of changingness into every aspect of his psychological life. He will therefore continue to be a continually changing person, experiencing with freshness and immediacy in each new situation, responding to its newness with real and accepted feelings, and construing its meaning in terms of what it *is*, not in terms of some past experience.

RECAPITULATION

I have tried to sketch, in a crude and preliminary manner, the flow of a process of change which occurs when a client experiences himself as being received, welcomed, understood as he is. This process involves several threads, separable at first, becoming more of a unity as the process continues.

This process involves a loosening of feelings. From feelings which are unrecognized, unowned, unexpressed, the client moves toward a flow in which ever-changing feelings are experienced in the moment, knowingly and acceptingly, and may be accurately expressed.

The process involves a change in the manner of experiencing. From experiencing which is remote in time from the organic event, which is bound by the structure of experience in the past, the client moves toward a manner of experiencing which is immediate, which interprets meaning in terms of what is, not what was.

The process involves a loosening of the cognitive maps of experience. From construing experience in rigid ways which are perceived as external facts, the client moves toward developing changing, loosely held construings of meaning in experience, constructions which are modifiable by each new experience.

The process involves a change in the self. From being a self which is not congruent with experience, the client moves through the phase of perceiving self as an object, to a self which is synonymous with experience, being the subjective awareness of that experience.

There are other elements, too, involved in the process: movement from ineffective to effective choice, from fear of relationships to freely living in relationship, from inadequate differentiation of feelings and meanings to sharp differentiation.

In general, the process moves from a point of fixity, where all these elements and threads are separately discernible and separately understandable, to the flowing peak moments of therapy in which all these threads become inseparably woven together. In the new experiencing with immediacy which occurs at such moments, feeling and cognition interpenetrate, self is subjectively present in the experience, volition is simply the subjective following of a harmonious balance of organismic direction. Thus, as the process reaches this point, the person becomes a unity of flow, of motion. He has changed; but, what seems most significant, he has become an integrated process of changingness.

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THE PRESENT SITUATION IN BRAIN PHYSIOLOGY

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DURING the past ten years, I have worked in physiological psychology. This term refers to a rapidly growing number of problems and procedures. Most firmly established is the interest in questions of localization. Excellent investigations have shown how loss of function in one part of the brain or another affects behavioral achievements. Equally interesting are the results of tests in which the brain is either locally stimulated or, as a whole, subjected to electric shocks. We are especially impressed by the discovery that stimulation of certain parts of the brain seems to be equivalent to an award and that of other parts to a punishment. Attempts to influence behavior by chemical action have so far not had equally satisfactory results. For, while the changes in question are often strong, it is still difficult to describe them in precise terms. But so great is the interest which the medical profession takes in such experiments that present shortcomings of the chemical approach will probably be overcome within the next few decades. Efforts to relate the chemical characteristics of individual brains to behavior are just beginning to yield promising findings.

While there can be no objection to these endeavors as such, physiological psychology should also be concerned with problems to which such investigations rarely refer. First of all, what exactly happens in the brain when certain psychological events take place? Even if we knew in detail what areas of the tissue are involved in particular achievements, we would still not be able to answer that question. Let me mention a simple example. To my knowledge, neither birds nor fishes have a visual cortex; and yet, the pattern vision of certain birds and fishes resembles that of the primates who have a visual cortex. Consequently, a given kind of process may occur in different locations; and, if this is true, we have to deal with a functional problem which studies of localization alone can not solve. My own work in physiological psychology refers to functional questions in this sense.

I do not believe that this part of our task can be entirely left to the neurophysiologists. For, much as I admire their actual achievements, we disagree on one major issue. At the present time, no evidence as to the nature of brain function can compare with our own, that is, the psychologists' evidence, and most of this the physiologists quietly ignore. They obviously do so in the conviction that the main principles of peripheral nervous function are also those of central function. Hence, since they know the former processes extremely well, they do not seem to expect that, when studying the brain, they might sooner or later need our help.

But the most important process of the peripheral nervous system is the nerve impulse. If brain function, too, consisted mainly of such impulses, then hosts of psychological facts could never be understood in physiological terms. Nerve impulses are particularly lonely events. While they travel along their fibers, each is almost, if not entirely, unrelated to the others. The principal characteristic of most psychological situations, on the other hand, is interrelation of their parts. Some such relations may be established by impulses which travel in transverse neurons from one place in the brain to another. But in many instances this explanation does not work because the interactions in question depend upon the specific nature of the various interacting facts. Nerve impulses which seem to be always events of the same kind can hardly represent the specific characteristics of one local fact at the place of another. Psychologists will also hesitate to admit that such different phenomena as perception with all its modes, memory, learning, attending to, feeling, thinking, and planning can all be derived from such a monotonous principle of action.

I do not, of course, deny that impulses travel in axons of the brain just as they do in peripheral fibers. I merely suspect that, in addition, there are other forms of brain action and that, from the point of view of psychology, such other actions are no less important than the impulses. Hypotheses about these further processes can be developed only by those who are familiar with certain parts of physics, with neurophysiology, and with the basic facts of psychology. I do not suggest that we return to what was once called brain speculation.

In recent years, my collaborators and I have formulated such an hypothesis and have then tried to verify it in physiological experiments (10–18). The psychological fact from which we started was the segregation of molar units or things in visual fields. It seemed to us a plausible assumption that,

when the brightness of a certain area differs from that of its environment, this area will be pervaded and surrounded by an electric current. The current inside and the current outside would flow in opposite directions. Functionally, therefore, a sharp discontinuity would be established at the boundary of the area.

A few years later, Wallach and I began to study the distorting effects which prolonged inspection of a visual object has upon this object itself and upon test objects afterwards shown in about the same region (17). I need not describe how we soon found ourselves in the good company of Gibson who had discovered similar effects in other instances (6). Our own experiments showed that the observed distortions follow a general rule. Test objects recede from the regions in which the previously inspected objects, and particularly their contours, have been seen—as though an obstruction had been established in these regions. Not only our own observations but also those of Gibson could be derived from this rule.

It was, of course, our next question whether such phenomena can be explained in terms of known physiological facts. Does any process cause obstructions in the nervous system? There is such a process. When a current flows through cells of the nervous system, it affects their surfaces. Where it enters the cells, it raises the local impedance and thus blocks its own way. Where it leaves the cells, the opposite happens. But since, after a short while, the former effect becomes stronger, the final result is an obstruction. Thus the current is weakened, and its distribution in the tissue changes. When it is interrupted, the affected tissue remains in its altered condition for considerable periods. Test currents which are now conducted through this region will, therefore, also deviate from their normal course. They will be weakened where the impedance has been raised, and their intensity will grow in less affected parts. Hence, the current as a whole will recede from the affected region just as, in a figural after-effect, a test object recedes from the area previously occupied by an inspection object.

Our assumption that activated parts of the visual cortex are sources of electric currents can, therefore, also explain the figural after-effects. Are we right in saying that the displacement of test objects in figural after-effects and the deflection of test currents in the neighborhood of obstructed tissue are remarkably similar facts? I once had an opportunity to demonstrate after-effects to a well-known physiologist who was not yet acquainted with these phenomena. After several observations, he turned to me and said with a smile: "Nice demonstrations of electrotonus, aren't they?" Electrotonus is an old name for the obstructions which currents establish in the nervous system.

When electrotonus was first discovered, it was caused by currents conducted through the tissue from the outside. Since, in our theory, this condition is supposed to be established by currents which issue from active parts of the tissue itself, we now had to demonstrate that there are such currents. This proved to be a difficult enterprise, but eventually we succeeded (12–18). At the present time, numerous records of visual and auditory currents are available. They were taken from intact human heads and from the exposed projection areas of cats. Occasionally, visual currents have also been registered from the brains of monkeys. I wish to thank Held, O'Connell, Neff, Wegener, Pribram, and Rosenblith for their invaluable collaboration.

Our findings agree with known facts of cortical localization and may therefore be regarded as reliable. The electrotonic action of the registered currents is also obvious. Continuation of the flow in given parts of the tissue soon reduces its intensity. So far, therefore, our factual evidence agrees with our expectations, and our theory of figural after-effects seems now to rest on fairly firm ground.

Figural after-effects are merely deviations from what we call normal perception. An explanation of these effects in physiological terms must therefore include statements about the processes underlying perception in general. Actually, our suggestion that cortical currents play an important part in normal visual perception preceded our work on figural after-effects. This thesis, however, has not been generally accepted, partly because it seems to be at odds with certain experimental findings.

Lashley and his collaborators (19) laid strips of gold foil on the visual cortex of one monkey and thrust thin wedges of the same material into the occipital lobes of another. It was assumed that these measures would greatly distort the distribution of visual currents. When postoperational tests showed no disturbance of the animals' vision, the authors concluded that perceptual organization can not be related to such currents.

I do not believe that this conclusion is justified. Strips of gold foil attached to the surface of the brain can cause no major disturbance of cortical

currents because the electromotive forces involved operate at right angles to those metallic conductors. Under these circumstances, only an exceedingly small part of the flow will be deflected into the very thin strips and, since this part, too, must return to the other side of the source, it can not follow them for more than a very short stretch. Wedges of gold foil thrust into the cortex will at first cause a stronger disturbance; but the very fact that now a highly concentrated flow must pass through these wedges will establish abnormally high electrotonic obstructions in the tissue around their tips, and soon the conductors may practically be sealed up.

Sperry, Miner and Myers (22), who worked with cats, took several steps to distort the cortical currents. In one experiment, they inserted tantalum wires into the visual cortex. In some animals, the tissue was sliced by vertical cuts in various directions; in others, small plates of an insulating material were pushed into the visual region. Extraordinary difficult discriminations between visual patterns which the cats had previously learned to perform were, on the whole, not very greatly disturbed by these radical measures.

I have difficulties in understanding this finding. It seems to me incompatible not only with my own views but also with generally accepted neurological evidence. For, autopsies revealed that, in several instances, the visual projection system including the geniculate bodies had been very severely damaged. Even during the initial training, the discriminations which the cats had to perform must have taxed their abilities to the utmost. How could their achievements remain similarly precise when much of the tissue necessary for vision had been destroyed? It is to be hoped that these experiments will soon be repeated. The authors realize that any material which is frequently used in animal experiments will gradually acquire particular characteristics which may serve as extraneous cues; but their statements in this respect do not answer one question: With very few exceptions, they always used the same pattern, an equilateral triangle, as the positive object, while up to 40 modifications of this triangle served as negative objects. Were correspondingly numerous copies of the positive object presented during the training and the final tests?

No comments on our work have so far been made by the neurophysiologists. I suspect that they do not like its general trend. Cortical currents or fields differ from the more popular nerve impulses in three respects. In the *first* place, they do not follow the all-or-none principle which holds for the impulses. The currents are graded processes; the degree to which their cortical sources are activated, and therefore also their own intensity, vary from one instance to another. Physiologists will hardly raise objections on this particular ground because they are quite familiar with graded events in the nervous system. Such events are known to occur at the surface of axons and also at synapses.

Secondly, unlike the nerve impulses and the electric rhythms of cortical cells, the currents shown in our records have not the form of short-lived waves; rather, they are quasi-steady states. But again, such states occur quite regularly at synapses and have been recorded in numerous physiological investigations. I will presently return to these studies.

A *third* difference between cortical currents and nerve impulses refers less to certain facts than to the interpretation of these facts. While the currents spread freely in the tissue as a continuous medium, nerve impulses follow prescribed linear conductors, the nerve fibers. But no physiologist will deny that many electric states of cells and fibers have fields which also spread beyond their sources. The field of nerve impulses has been measured at considerable distances from the active fibers (20). The alpha-rhythm of cortical cells can be registered from intact human heads only because this rhythm establishes corresponding currents not only in the cortex but also in the skull and the scalp. Or take the evoked potentials which announce the first arrival of afferent impulses in the projection areas of the brain. It has been shown that these potentials, too, can be recorded from the intact head. Physiologists accept these facts, but they do not seem to regard them as very important. The reason is obvious and has once been clearly formulated by Adrian (2). To the extent to which neural processes are located in or on histological elements, their behavior in the tissue is prescribed by the arrangement of these elements; and, since this arrangement appears to be most orderly and practical, the distribution of corresponding functions will be equally precise and practical. On the other hand, the currents which spread around the active elements are not subjected to such restrictions. Hence, it is often assumed that, if they do not cause confusion and disorder, their influence on the tissue and on other processes must be negligible.

Every now and then, a physiologist discovers facts which can not be reconciled with this interpretation of neural action. The fields of activated elements do influence processes in other elements. For instance, under certain conditions the cat's spinal cord exhibits a regular rhythm. Bremer (4) has demonstrated that this activity is synchronized from one end of the cord to the other. The author himself remarks that the rhythm spreads with a speed far beyond the speed of ordinary neural transmission and that the interaction involved must therefore be mediated by electric fields of the active cells. To prove that this is true, he even made a sharp cut through the cord which left its two parts unconnected by histological elements, that is, connected only by physical contact. Afterwards, the rhythms on one side and the other of the cut were just as nicely synchronized as they had been before. Similar observations made by Gerard and Libet (5) in their work on the frog's brain are, of course, generally known. It follows that neural interactions can be maintained quite independently of messages which travel along fibers.

Even more important is another function of neural fields. In 1938, Barron and Matthews (3) published a most important paper on the steady potentials which impulses establish at dorsal and ventral synapses in the spinal cords of frogs and cats. They realized that such potentials must be sources of equally steady currents, and they also discovered that these currents have a remarkable effect upon the cells through which they pass. As soon as their intensity has reached a certain threshold value, they throw these cells into rhythmic action, which is then, in the form of impulses, propagated along the axons of the cells. To my knowledge, all records of ventral synaptic potentials and their currents show this effect. Consequently, while occasionally transmission at a synapse may be a direct effect of the impulses which arrive in presynaptic fibers, this transmission is for the most part mediated by synaptic currents. Under the circumstances, one can no longer maintain that the currents of the nervous system are mere epiphenomena.

Steady potentials of neural structures have been demonstrated by several physiologists. Adrian (1) has shown that the electric response of a beetle's optic ganglion is a steady state or current. Quite recently, Goldring and O'Leary (7, 8) have registered steady currents from the visual cortex of the rabbit and have thus confirmed our own findings. There seems to be little doubt as to the origin of cortical currents. The steady spinal currents discovered by Barron and Matthews issue from activated spinal synapses. Most probably, the sources of perceptual currents are activated synapses in the cortex.

Let me add a few remarks about problems in my field which ought to be solved next. First of all, figural after-effects have been demonstrated not only in the frontal plane but also in the third dimension. Years ago, Gibson (6) discovered such effects in kinesthesis, and we studied further examples in this modality. In addition, figural after-effects occur in passive touch, and most probably also in hearing. Even certain inhibitions which tend to impede learning may partly be caused by what we have called "satiation." It has not yet been possible to apply our physiological assumptions to all these phenomena. The reason is simply that nobody knows the physiological facts which underlie visual depth, kinesthesis, and learning. Naturally, we can not tell how electrotonus affects these processes until their nature as such has been revealed. Thus, we have three new tasks which follow from the study of figural after-effects.

As though this were not enough, there is another problem to be solved in this field. Ever since Gibson's first discovery, it has been known that, under certain conditions, figural after-effects are not prevented by eye movements of the subject. Again, when many objects of a given kind slowly move across the visual field while the subject fixates a mark, the moving pattern still causes a figural after-effect, and this effect corresponds to the shape of the moving objects. During Ivo Kohler's well-known experiments (9), the subjects could also move their eyes as they wished; and yet, the resulting specific adaptations had much in common with figural after-effects. Now, if the obstructions established under such conditions were all localized at exactly the same neural level, the statistical result would be a homogeneous layer of satiation which could cause no specific after-effects. It seems to follow that the location of obstructions which arise at different times can not be precisely the same —in other words, that time is spatially represented in the brain just as it is in the geological strata on the surface of the earth. This may seem to be a bold hypothesis; but it is the only assumption which can explain such remarkable facts. Incidentally, there is also no other explanation for certain facts in memory. Memory traces, too, remain

individual entities; although, statistically speaking, they must be established everywhere and would therefore form a homogeneous layer of affected tissue if their location in detail did not depend upon the time of their origin. As a further task, we must therefore try to discover how the temporal dimension is represented in the tissue.

These are fairly formidable problems. Another task which is again related to the figural after-effects does not look quite so forbidding. I mentioned that the term electrotonus is commonly used as a name for the obstructions caused by currents. Actually, as we have seen, currents affect the surfaces of cells in two ways: they establish an obstruction only where they enter cells, and they lower the local impedance where they leave them. The more precise name for the former change is anelectrotonus, and the latter is called catelectrotonus. Strictly speaking, therefore, our theory of figural after-effects is mainly concerned with anelectrotonic action.

But are we allowed to make use of this particular action alone and simply to ignore that cortical currents must also establish catelectrotonic changes? Under the conditions which give rise to figural after-effects we can safely do so, because under these conditions the anelectrotonic changes are particularly strong. There are, however, situations in which the catelectronic effects ought to be stronger, and where this happens the flow of currents will be facilitated rather than impeded. As a consequence, tissue in the catelectrotonic state must attract test currents and, if these currents are those of perceptual objects, these objects must appear displaced *toward* the tissue in question.

Physiological evidence permits us to predict under what circumstances currents and objects will be attracted by certain places in their neighborhood. Best conditions will be those in which perceptual objects just appear or disappear, and also those in which one object appears while the other is just disappearing. On the other hand, when perception begins to be stable, catelectrotonic attraction will soon be overcome by anelectrotonic repulsion. We all know perceptual facts which only arise under the conditions favorable to catelectrotonic effects. I am referring to the various forms of apparent movement. But certain achievements of memory may also be related to catelectrotonic facilitation. At the present time, our studies are mainly concerned with these two issues.

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AMERICAN PSYCHOLOGICAL ASSOCIATION

DISTINGUISHED SCIENTIFIC CONTRIBUTION AWARDS
1957

DISTINGUISHED Scientific Contribution Awards of the APA were presented to Edward C. Tolman (in absentia), Carl I. Hovland, and Curt P. Richter (in absentia) during special ceremonies at the 1957 APA Annual Meeting. This second group of recipients joined Kenneth W. Spence, Carl R. Rogers, and Wolfgang Köhler, to whom similar awards were made in 1956, in being honored by the APA for contributions to the development of scientific psychology. Each recipient was presented, in person or in absentia, with a formal citation of his outstanding scientific contribution to psychology and an honorarium of $1,000. Each will give his award address at the APA Annual Meeting in Washington in 1958.

PRESENTATION OF THE AWARDS

The presentations were made by Theodore M. Newcomb:

"Two years ago, at our sixty-third Annual Convention in San Francisco, it was decided to make three awards, annually, for distinguished scientific contributions to psychology. One year ago, at Chicago, the recipients of the first three awards were named, and only this morning their addresses, as award winners, were delivered. Each award carries with it, not only the obligation to deliver a lecture one year later, but also a check for $1,000 and a scroll on which is inscribed the citation which describes the contribution for which the award has been made.

"The selection of award winners is made, each year, by a special committee elected by the Council of Representatives. This year's Chairman of the committee was Deal Wolfle, who has asked me to announce the awards and present the citations, in view of his unavoidable absence.

"There are two primary ground rules by which each year's committee is guided, by instructions of the Board of Directors, in making its decisions. First the awards are made not merely for a single experiment or book, nor in terms of a general career-long impact of general nature, but in recognition of a specifiable contribution, continued by way of a number of publications, over a period of time, to scientific psychology. Secondly, in any one year an award should be given to not more than one person primarily identified with work in the same specialized topic (such as sensory, socialization, or learning); or with the same specialized material (such as animals, children, or abnormals); or with the same specialized technique (such as projective tests, mathematical models, physiological analysis); or with the same particular application (such as educational, industrial, clinical).

"In accordance with these instructions, your committee is pleased to announce the following awards, for the contributions as noted in the citations that I shall read: Edward Chase Tolman, Carl Iver Hovland, and Curt Paul Richter."

EDWARD CHASE TOLMAN

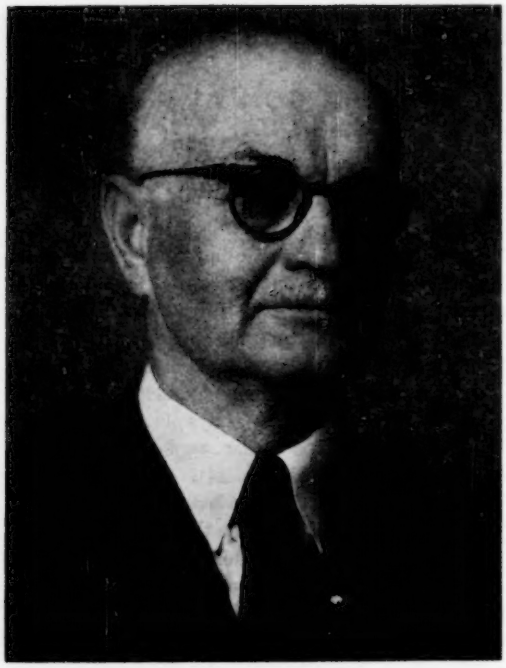
Citation

"For the creative and sustained pursuit of a theoretical integration of the multifaceted data of psychology, not just its more circumscribed and amenable aspects; for forcing theorizing out of the mechanical and peripheral into the center of psychology without the loss of objectivity and discipline; for returning man to psychology by insisting upon molar behaviorism purposely organized as the unit of analysis, most explicitly illustrated in his purposive-cognitive theory of learning."

Biography

Edward Chase Tolman was born in West Newton, Massachusetts, April 14, 1886. He graduated from the Newton High School in 1906 and received a BS in electrochemistry from the Massachusetts Institute of Technology in 1911. He moved to Harvard and to psychology, obtaining his MA in 1912, his PhD in 1915.

Tolman was an instructor at Northwestern University from 1915 to 1918, then joined the faculty at the University of California in 1918, where he has remained save for the period August 1950 to January 1953 (during the loyalty oath controversy). He was attached to the Office of Strategic



EDWARD CHASE TOLMAN

Services during 1944–1945. He was Visiting Professor of Psychology at the University of Chicago during the Spring of 1951. He became Professor Emeritus in 1954.

Tolman became a member of the Society of Experimental Psychologists in 1930; the National Academy of Sciences, 1937; the American Philosophical Society, 1947; the American Academy of Arts and Sciences, 1949; Honorary Fellow, British Psychological Society, 1954. He received the honorary degree of Doctor of Science from Yale University in 1951, and from McGill University in 1954.

Tolman was chosen for the following distinguished lectureships: the Faculty Research Lecture, University of California, 1947; the Kurt Levin Memorial Award Lecture, 1949; the Penrose Lecture, American Philosophical Society, 1951.

He was President of the APA in 1937; President of the Society for the Psychological Study of Social Issues, 1940; Vice-President of the American Association for the Advancement of Science, 1944; President of the following divisions of the APA: Division of General Psychology, 1953; Division of Theoretical-Experimental Psychology, 1946; Co-President, International Congress of Psychology, 1954.

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CARL IVER HOVLAND

Citation

"For his original and provocative contributions to the scientific study of persuasive communications and the modification of beliefs and attitudes. Combining a sensitive use of controlled experimentation with penetrating logical analysis, he has done much to isolate the major factors at work when an individual is confronted with the complex informational input of a persuasive argument. By judicious use of psychological theory, he has been able to relate this area of social psychology to basic investigations of the higher mental processes. His work has been of central importance in advancing attitude research from the early stage of merely demonstrating that changes can be produced to the point of making predictions about when and where they will occur. His work has provided a convincing demonstration of the values of a sustained and integrated program of research."

Biography

Carl Iver Hovland was born in Chicago, Illinois, on June 12, 1912. He received both his bachelor's degree (with highest distinction) and his master's degree at Northwestern University. In 1933 he began his work for the doctorate at Yale University, where, except for a leave of absence between 1942 and 1945, he has since remained. He became an instructor upon the completion of the PhD in 1936, Assistant Professor the following year, Associate Professor in 1942, and Professor in 1945. For the next six years he was Chairman of the Department and Director of the Laboratory of Psychology. He

CARL IVER HOVLAND

is now Sterling Professor of Psychology, Director of Graduate Studies, and a Fellow of Silliman College, at Yale.

Beginning in 1935 and continuing through 1940 he published from four to six experimental studies a year, on a wide range of problems. Perhaps the most notable of these were his series of reports on the generalization of conditioned responses and on rote learning. During this period, also, he was one of the authors (with J. Dollard et al.) of *Frustration and Aggression* and (with C. L. Hull et al.) of *A Mathematico-Deductive Theory of Rote Learning.*

During the period of American participation in World War II, he served as Director of Experimental Studies and Chief Psychologist in the Research Branch of the War Department's Information and Education Division. His was the major responsibility for planning and executing the research later reported (jointly with A. A. Lumsdaine and F. D. Sheffield) under the title, *Experiments in Mass Communication,* which was issued as Volume III of the series, *Studies in Social Psychology in World War II.* From his participation in this research, much of which dealt with the effects of showing films to groups of American soldiers under controlled conditions, grew his subsequent interest in the more general problems of the psychological aspects of persuasive communication. Many research reports and two major publications (*Communication and Persuasion,* with I. L. Janis and H. H. Kelley, 1953; and *Order of Presentation in Persuasion,* 1957) have already come from the Yale Communication Research Program, of which he is the Director.

Hovland has been a member of the Board of Directors of the Connecticut Psychological Association, of the Eastern Psychological Association, and of the American Psychological Association. He has been a Director of the Social Science Research Council and of the Vocational Counselling Service, a member of the Editorial Board of *Annual Review of Psychology,* and a consulting editor to several psychological journals. He has been consultant to many governmental and educational agencies, industrial organizations, and philanthropic foundations. He is an ABEPP Diplomate in Industrial Psychology and a Fellow of the American Association for the Advancement of Science, of the American Philosophical Society, of the Society of Experimental Psychologists, of the American Academy of Arts and Sciences, of Phi Beta Kappa, and of Sigma Xi.

Scientific Publications

1934

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CURT PAUL RICHTER

Citation

"For his pioneering investigations of physiological mechanisms affecting behavior. His researches have given rise to a new concept of homeostasis and enriched our understanding of the reciprocal relationship between behavior and the organism's internal environment. His painstaking and rigorous studies of body chemistry as it influences behavior have added a new dimension to physiological psychology."

Biography

Curt Paul Richter was born in Denver, Colorado, on February 20, 1894. He took his BS degree at Harvard in 1917 and his PhD in psychology at Johns Hopkins University in 1921. He was Assistant in Psychobiology at the Phipps Psychiatric Clinic of the School of Medicine, Johns Hopkins, during 1921 and 1922. He became Instructor in 1922 and held this position until 1925. From 1925 to 1929, he was an Associate; from 1930 to 1931, a Lecturer; and in 1932, he became an Associate Professor. Since 1921 Richter has been Director of the Laboratory of Psychobiology at the Phipps Clinic.

Richter was awarded the Warren Medal by the Society of Experimental Psychologists in recognition of his researches upon factors regulating the selection of diets by animals.

He has acted as consultant to OSRD, USPHS, and The Surgeon General's Office.

He is a member of the National Academy of Sciences, the American Academy of Arts and Sciences, the American Psychological Association (Fellow), the American Physiological Society, the Society for Experimental Biology and Medicine, the American Neurological Society, and the Association for Research in Nervous and Mental Disease.

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Decreased appetite for alcohol and alcoholic beverages produced in rats by thyroid treatment. In *Proc. conference on neuroendocrinology*. New York: Academic Press.

RULES OF SELECTION

In his introductory statement, quoted above, Newcomb described two rules that the selection committee was instructed to observe in selecting award winners. The second of these rules prohibits making an award in any one year to more than one person who is primarily identified with work on the same specialized topic, with the same specialized material, with the same specialized technique, or with the same particular application. The

committee has had extended correspondence considering this rule and the possible desirability of rescinding it. It should be stated immediately, however, that the committee is unanimous in endorsing the principle that the persons selected for awards in any one year should be so chosen as to represent a wide range of psychological problems, methods, and interests. The awards are intended to honor psychologists whose work merits such distinction, but there is a larger purpose of promoting, on a broad basis, the development of distinguished scientific contributions to psychology. We believe that any conscientious selection committee would take this broader objective into consideration and would feel compelled to avoid making awards in the same year to research workers of closely similar interests.

The question arises, however, as to whether the restriction is too great. The field of learning has been a popular and fruitful one in psychology. If a psychologist who is primarily identified with the field of learning is selected for an award in one year, all other psychologists so identified are excluded for that year. Animals have been useful subjects in a wide variety of psychological research. If one of the award winners has made major use of animals in his studies, all other "animal" psychologists are excluded for that year. These categories overlap; the selection of a person who has worked on animal learning problems excludes from consideration psychologists who have either worked on learning problems or with animal subjects.

Some of the members of this year's committee believe that these restrictions are too great and that they will quickly force succeeding selection committees to search for psychologists who are eligible rather than to award those whose contributions have been of greatest significance. The committee is not unanimous on this point, however. A proposal that we recommend to the Board of Directors that the restriction be removed was approved by three members and opposed by two.

How the rule is phrased is an important matter for the selection committee, but the more basic matter is to reach agreement on the goals of the award program and on the values to be emphasized in its administration. Wisely, there is each year a complete change in the membership of the selection committee. This problem is, therefore, for consideration by next year's committee, by the Board of Directors, and by the members of the discipline on whose behalf the awards are made.

COMMITTEE FOR SELECTING THE RECIPIENTS OF AWARDS FOR OUTSTANDING CONTRIBUTIONS TO SCIENTIFIC PSYCHOLOGY

FRANK BEACH
PAUL HORST
JEAN WALKER MACFARLANE
THEODORE M. NEWCOMB
DAEL WOLFLE, *Chairman*

AMERICAN PSYCHOLOGICAL FOUNDATION

GOLD MEDAL AND DISTINGUISHED SCIENTIFIC WRITING AWARDS
1957

At the APA Annual Meeting in New York on Setember 2, 1957, the American Psychological Foundation announced the recipients of the APF Gold Medal Award and of the APF Distinguished Science Writing Award. Presentations were made by E. Lowell Kelly, President of APF.

GOLD MEDAL AWARD

"One year ago, the American Psychological Foundation named Robert Woodworth as the first recipient of its annual Gold Medal Award for a distinguished and protracted history of scientific and scholarly accomplishment. Today the foundtion had planned to honor similarly another great man in psychology, but unfortunately he is no longer among us to receive the honor. The Board of Trustees of the foundation decided against a policy of awarding this medal posthumously and also against naming another award winner in his place this year. Under the circumstances, it is suggested that we honor by a brief period of silence the memory of the one who, if he had lived, would have been named today as the recipient of the foundation's second Gold Medal Award: LEWIS MADISON TERMAN."

DISTINGUISHED SCIENCE WRITING AWARD

"In September 1956, the American Psychological Foundation announced intention of awarding from time to time an Award for Distinguished Science Writing in the Field of Psychology, to consist of a scroll and a check for $500. It is my pleasure today to announce the first recipient of this award: ERNEST HAVEMANN.

"Mr. Havemann, I am indeed happy to present you the American Psychological Foundation's first Award for Distinguished Science Writing in the Field of Psychology. So that all members of this audience may be aware of the basis of this award (and because I fear you would be too modest to read it aloud), I shall read the scroll before handing it to you."

Citation

"To Ernest Havemann: For his articles on psychology in *Life* magazine, January 7, 1957 to February 4, 1957. The award is designed to foster popular understanding of psychology; and Mr. Havemann's series was judged to be outstanding, not only for accuracy, lucidity, and interest, but for truly exceptional grasp of the diversity of modern psychological research and services. We feel his work reflects great credit upon his profession and upon the ability of the mass media to correctly interpret difficult subjects to the millions. Mr. Havemann's writing has carried a sound perspective on psychology into more American homes than any other single publishing enterprise, and so we honor him as a great teacher of psychology."

ERNEST HAVEMANN

Biography

Ernest Havemann, a native of St. Louis, received his AB degree at Washington University and for a time gave some thought to becoming a psychologist. In fact, he went as far as completing his master's degree with John Nafe in 1934. The following year, however, found him beginning his career in journalism, first as a reporter and rewrite man for the *St. Louis Star Times* and later for the *St. Louis Post Dispatch*. In 1940, he served one year as Washington correspondent for *Time* magazine. During World War II, he was a *Time* Editor and writer. After the war, from 1946–1955, he held a similar position on the staff of *Life* magazine. Since 1955, he has engaged in free-lance writing for several well-known magazines. He is the co-author of two books both published in 1952: with Patricia Salter West, he wrote *They Went to College;* and with George Lowe, *I Never Thought We'd Make It.*

Acceptance

In accepting the award, Havemann said, in part:

This award pleases me so very much that, instead of a mere routine thank you, I should like to spend a few words —but I promise you, not more than three or four minutes' worth—telling you why.

One of my first assignments as a young reporter was to interview a visiting doctor in St. Louis—a man who had just made some important discoveries, as I remember it, in the field of vitamins. I found him and his wife in a hotel room, and he spent 20 minutes or so telling me about his work. Then I left. Outside the door I heard his wife say: "Darling, that was a wonderfully clear explanation you gave that boy." And the doctor said: "Thank you, my dear, but wait till you see how he louses it up."

That was pretty much the general attitude of scientists and other men of learning toward journalism in those days, just 20 years ago. The fault was on both sides. Journalism was not then equipped to understand and interpret the scientist. For his part, the scientist was often somewhat cavalier.

Well, I have been thinking about this incident because it was in such complete contrast to what happened on the magazine articles for which you have so generously honored me. Journalism has quite a stable of editors nowadays who have far too much respect for science ever to "louse up" the facts. And certainly a great many scientists went to considerable trouble in this case to help me in every possible way. There was Clifford T. Morgan, who was *Life* magazine's official adviser. There was also the entire staff of the American Psychological Association—and indeed what seemed at times like its entire membership. There were far too many people to even begin to mention here.

Science worked very closely with journalism, and the result was that science was pleased. Or at least many of you were. And that is why I have been so gratified by this award.

Surely science and journalism need the closest of relations. There was a time, not many years ago, when mankind seemed to be lost for lack of the facts. Now the facts are piling up at a gratifying rate—but the bulk of mankind still does not know about them. You psychologists probably don't meet them; but you would be surprised at the number of people I see in the course of my work every year, some of them terribly influential in the arts or in politics, who are ardent believers in things like astrology, prenatal influence, asafoetida, and black cats. The great curse of our day may be that we are accumulating facts in the laboratories much faster than mankind has been able to learn about and act upon them.

But of course this is something that all of you have surely thought about many times, and this is hardly the place to expand upon it.

The point I wanted to make is that I am particularly pleased with the award because it is surely a step in the right direction. It represents a considerable advance in psychology as a science of living—which is what every science should be in the ultimate sense—that you should have set the award up at all and that so many of you should have been so willing as individuals to help me win it.

I hope that we meet—you and I, and science and journalism in general—many times again.

AMERICAN PSYCHOLOGICAL FOUNDATION

J. P. GUILFORD
J. McV. HUNT
E. LOWELL KELLY, *President*
O. HOBART MOWRER
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LAURANCE F. SHAFFER
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ROBERT R. SEARS

CLINICAL PSYCHOLOGY IN BRITAIN

ARTHUR SUMMERFIELD [1]

General Secretary, British Psychological Society

THE course of professional development in other countries may often be examined with advantage, as was suggested in the article on "Clinical Psychology in Britain" (2) which appeared in the *American Psychologist* this last year. Since this earlier article was based on the situation as it was in 1954, it may be of interest to American psychologists to see the pattern of events traced more fully up to date.

Clinical psychology in Britain is a postwar development. It has depended upon the farsightedness of a number of psychologists who were at work during and before the war. But when the war ended few psychologists were working in this field. In essentials the development of clinical psychology has therefore been that of a new field of application rather than the expansion of one which was already well established on a smaller scale. It has differed markedly in these respects from occupational, industrial, and educational psychology.

Two other factors have been influential. Its development has occurred (*a*) in a time of increasing demand for applied psychologists of all kinds; and (*b*) almost entirely within the context of the National Health Service, brought into being on July 5, 1948 by the National Health Service Acts of 1946 and 1947. These environmental factors have therefore to be taken into account.

THE NATIONAL HEALTH SERVICE

To see where the clinical psychologist fits in, some conception of the National Health Service as a whole is needed (cf. 1). In administrative structure it is extensively decentralized. The Treasury exercises overall financial control. There are two central authorities, the Ministry of Health (for England and Wales) and the Department of Health for Scotland. Their primary functions are general supervision and coordination of the three distinct systems of which the service is composed: (*a*) the welfare health services, like maternity care and child welfare, administered by local government authorities (county and county borough councils); (*b*) the general practitioner services administered through specially established local executive councils; and (*c*) the hospital service. It is in the last of these that nearly all clinical psychologists are employed.

The hospital service covers hospitals of all kinds and the clinics attached to them: general hospitals, teaching hospitals associated with medical schools, specialized hospitals, mental hospitals, and hospitals for the mentally defective. The decentralized organization is primarily regional. Within regions, groups of hospitals are administered together—a group in some cases being a single large hospital, in others covering several hospitals differing in size and function. Mental hospitals may be grouped with hospitals of other kinds, but have tended not to be. The teaching hospital groups occupy a distinct position. They have a special form of administration (Board of Governors) directly responsible to the central authority and not subject to regional supervision. At the same time a principle which was fundamental to the establishment of the regions was that the hospital services within each should be associated with at least one medical school and teaching hospital. There are 14 regions in England and Wales, including 4 metropolitan regions for the Greater London area with its high concentration of teaching hospitals, and 5 in Scotland. The South-west Metropolitan Region includes the only teaching hospital group for psychiatry and neurosurgery—the Bethlem Royal and Maudsley Hospitals Group in association with the Institute of Psychiatry of the University of London.

Administration within each region is at the two levels of Regional Hospital Board and Hospital Management Committees. Service on either of these kinds of body is honorary. Their members are drawn locally from academic, professional, busi-

[1] I am indebted to the Chairman, the Secretary, and other members of the committee of the Adult Section of the Committee of Professional Psychologists (Mental Health) of the British Psychological Society, and to the President and the Deputy President of the society, for valuable comments and suggestions.

TABLE 1

SALARY SCALES FOR CLINICAL PSYCHOLOGISTS IN THE NATIONAL HEALTH SERVICE, 1952–1957

Grade	PTA 10 (1952)			PTA 18 (1953)			PTA 27 (1954)			PTA 39 (1956)		
	Entry to Grade	Annual Increment(s)[a]	Limit of Scale	Entry to Grade	Annual Increment(s)[a]	Limit of Scale	Entry to Grade	Annual Increment(s)[a]	Limit of Scale	Entry to Grade	Annual Increment(s)[a]	Limit of Scale
Psychologist[b] "Entry Grade"	£380	£25(4)	£480	£415	£25(4)	£515	£415	£35(1) £50(1)	£500	£550	£25(4)	£650[c]
"Basic Grade" (at age 25)	£530	£25(6) £30(3)	£770	£565	£25(2) £30(3)	£805	£575	£30(8) £25(1)	£850	£700[d]	£30(8) £35(1)	£975
Senior Psychologist	£810	£40(9)	£1,170	£845	£40(4) £35(4) £25(1)	£1,170	£875	£40(3) £35(6)	£1,205	£1,000	£40(7) £45(1) £50(1)	£1,375
Top Grade Psychologist[e]	£1,300	£75(4)	£1,600	£1,300	£75(4)	£1,600	£1,335	£75(4)	£1,635	£1,500	£75(4)	£1,800

[a] The figure in brackets after each amount is the number of such annual increments.

[b] The description and conditions attaching to this grade have changed several times; the terms "Entry grade" and "Basic grade" are not official terms (see text).

[c] Amended from £550–£25(3)–£625(PTA 39) to £550–£25(4)–£650 by PTA 52 (1957).

[d] Appointments below age 25 are now envisaged with abatement of the salary on entry to the grade by £30 for each year below 25.

[e] The figures shown for entry to the Top Grade are minimum salaries, actual salaries are subject to upward adjustment in individual cases; the four increments of £75 are, however, fixed.

ness, and other walks of life, as well as from the medical profession. Each Regional Hospital Board is responsible for the general planning and supervision of hospital services in its region and for the allocation of finances to the Hospital Management Committee in charge of each hospital group. Between them, these bodies are responsible for appointments. Senior medical appointments are made by the Regional Hospital Board. All other appointments are made by Hospital Management Committees. Hospital Management Committees are therefore the "employing authorities" for clinical psychologists.

Notable as an exception to the general pattern of decentralization are the national arrangements on salaries, qualifications, and conditions of service. They apply to all kinds of staff: medical, technical, nursing, clerical, and so on. The machinery for negotiating them is based on that developed in the Civil Service. There are nine negotiating bodies—Whitley Councils—for different groups of staff, and an additional general one. The "management side" of each council is composed of representatives of the central departments (Ministry of Health and Department of Health for Scotland), the Regional Hospital Boards, and the Boards of Governors of the Teaching Hospital Groups. The Association of Hospital Management Committees now also has one seat on each council. It was not at first so represented, in spite of the position of Hospital Management Committees as employers of all except senior medical staff. The "staff side" consists of representatives of staff organizations, trade unions, and professional associations. Agreements negotiated in the councils are published as official circulars. Clinical psychologists come under "Professional and Technical Whitley Council A," as do other nonmedical, graduate scientists, e.g. physicists and biochemists. Agreements concerning these

professional groups are included in the "PTA" series of official circulars.

CLINICAL PSYCHOLOGY IN THE NATIONAL HEALTH SERVICE

The first PTA Circular, No. 10, on the qualifications, conditions of service and promotion, and salaries of full-time clinical psychologists in Health Service was issued on February 7, 1952. There have been four others, Nos. 18 (February 28, 1953), 27 (February 16, 1954), 39 (July 24, 1956), and 52 (September 11, 1957). Additional circulars have been concerned with part-time appointments.

Table 1, shows the progressive changes in salaries specified in the five main circulars. The table also shows that from the beginning three major grades of clinical psychologist have been distinguished, with a subdivision into two of the "Psychologist" Grade. (Table 2 is included for comparison. It gives the general level of salaries paid to academic staffs of British universities, including psychologists, in 1952 and 1957). Salaries apart, it is in relation to appointments in the Psychologist Grade that the terms of the successive circulars have principally differed. Conditions of appointment in the senior grades have hardly changed at all.

The *Top Grade for special posts* has throughout applied "to a psychologist occupying a post of exceptional resonsibility"; establishment of Top Grade posts has required the sanction of the central authority (Minister of Health or Secretary of State for Scotland, as heads of the respective departments), which has in this matter the advice of a small committee of psychologists. Subject to the sanction of the central authority, employing bodies (Hospital Management Committees) have been enabled to fix salaries at or above the prescribed minima. All Top Grade appointments are considered individually. "Exceptional responsibilities" are not defined; they might therefore be, for example, in practice, research, supervision and training, or some combination of these.

The *Senior Psychologist Grade* has all along been for posts of greater than usual responsibility; duties "might or might not include supervision of other psychologists" (permitting appointments to be made in this grade also to posts whose responsibilities were other than supervisory in nature). It has always been considered that the supervisor of psychologists entering the service should be a Senior Psychologist.

The *Psychologist Grade* is the grade to which basic qualifications and training and conditions of entry relate directly. On this account it has been, and continues to be, the subject of the principal changes. In one way or another, it has from the start been divided into two categories. For convenience these categories have been distinguished in Table 1 by the unofficial terms "entry" and "basic" divisions of the Psychologist Grade, since the official usage has varied. It has been a basic premise that a clinical psychologist in the basic Psychologist Grade might work on his own in an independent appointment.

From the outset the conception of the requirements for appointment in this grade, in addition to a basic education in general psychology, has been the combination of a training in clinical psychology with actual experience of work in clinical psychology in the National Health Service or elsewhere. This broad conception with its twofold emphasis on training and experience commands the general support of psychologists and continues to prevail. At the same time, the sense in which it does so depends upon the way in which training and experience are themselves conceived. The developments of the past ten years in this connection are evidently fundamental and are next considered.

TABLE 2

SALARY LEVELS OF UNIVERSITY TEACHERS IN 1952 AND 1957

Grade of Appointment	1952			1957*		
Assistant Lecturer	£400	£50(2)	£500	£700	£50(3)	£850
Lecturer	£500	£50(12)	£1,100	£900	£50(9)	
					£75(4)	£1,650
Senior Lecturer and Reader	Varying maxima up to £1,600			Varying maxima up to £2,150		
Professor	£1,600 upwards			£2,300 maxima up to £3,000		

* Wightman (3).

ENTRY TO THE NATIONAL HEALTH SERVICE AS A CLINICAL PSYCHOLOGIST

The definitions, gradings, salaries and conditions of service set out in this agreement are for psychologists holding an honours degree in psychology of a University of England, Wales, Scotland or Ireland, or such other qualification as may be approved by the Minister of Health or the Secretary of State for Scotland.

The basic statement of qualifications took this form in 1952 (PTA Circular 10), as it continues to do. A psychologist so qualified but lacking previous experience in clinical psychology could be appointed in the entry grade. The emphasis was on "in-service" training as the approach to specific training in clinical psychology.

In considering this approach, it is relevant to take account of the conditions prevailing at the inception of the National Health Service to which reference has been made earlier. It is also to be considered that honors degrees in psychology, while not involving specific training in any field of application, nevertheless represent a high level of specialization in the subject. A counterpart of this is the tradition in all disciplines that higher degrees are awarded for research. Thus the PhD is awarded on the basis of an original research thesis; there is no advanced course work and no written examination. The same tradition attaches to master's degrees, but their award is sometimes dependent, wholly or in part, upon written and practical examinations. Postgraduate (i.e. graduate) diplomas and certificates have been the form of qualification awarded by universities in connection with courses involving actual practice, as well as instruction in underlying principles and research methods. It is also an established tradition that a person needs to have completed a proving period "on the job" before he can be reckoned fully competent for work in an applied field, however extensive his formal academic qualifications. Longer or shorter probationary periods, graduate apprenticeships, or terms of inservice experience under supervision are therefore customary in widely differing fields, e.g. engineering and technology as well as medicine.

During the past ten years, clinical psychology has come to show the influence of all these approaches:

1. Two institutions within the National Health Service have specialized in the inservice training of clinical psychologists: the Tavistock Clinic in London and the Department of Psychological Research at the Crichton Royal Institution in Scotland. Students taking these courses have already been admitted to the entry grade of National Health Service clinical psychologists. Inservice training is not, however, limited to these institutions, but may be given in any hospital.

2. Beginning in 1946, training in clinical psychology for honors graduates in psychology was developed in the Psychology Department of the University of London Institute of Psychiatry at the Maudsley Hospital. The course, extending over one year, was officially recognized by the university in 1950, since when a diploma has been awarded (the University of London Academic Postgraduate Diploma in Psychology, Section D, Abnormal Psychology). Students taking this course have been full-time, fee paying university students.

3. The Bethlem Royal and Maudsley Teaching Hospitals Group is, as mentioned earlier, in the South-west Metropolitan Region of the National Health Service. Advantage has recently been taken of this relation for trial of an arrangement whereby three clinical psychologists admitted to the entry grade within the region each year are seconded for their first year to the Maudsley Hospital to take the university diploma, while in the pay of the National Health Service. On successful completion of the course, they remain in the entry grade for a further period (normally a year) during which they work under the supervision of Senior Psychologists in the region.

4. Arrangements are currently being made to start a postgraduate course in clinical psychology by the Psychology Department of Edinburgh University, with the collaboration of other university departments and the Regional Hospital Board. It is expected that the first students will be enrolled in October 1958.

These developments have so far been reflected in a number of modifications to official arrangements. It was envisaged in 1952 (PTA Circular 10) that an honors graduate entering the National Health Service as an Assistant Psychologist might be "eligible to enter the grade of Psychologist" after "at least one year of training in clinical psychology, either under a Senior Psychologist or at a recognized training course, plus other appropriate experience"; he might, however, spend up to four years in the grade. In 1954 (PTA Circular 27) the grade of Assistant Psychologist ceased to be distinguished as the entry grade. Instead, the Psychologist Grade

was divided into two parts, of which the first three years were to be a probationary period leading to a proficiency bar. To pass the bar into the basic part of the Psychologist Grade, the probationer was required to "obtain a certificate of proficiency from the employing authority." If he did not do so at the end of three years, he was entitled to two further reviews at six-months intervals. The "employing authority" for a clinical psychologist, as has been explained, is a Hospital Management Committee. Psychologists were fully alive to the difficulties potential in this arrangement, and recommendations to the Ministry of Health were made on the point by the British Psychological Society. In 1956 (PTA Circular 39, currently operative), the requirement of a certificate of proficiency from the employing authority was deleted, and the probationary period conceived as follows:

> Before taking up an independent post in the National Health Service a psychologist shall normally have attained the age of twenty-five years and shall have satisfied one of the following conditions:
>
> (*i*) He shall have served for three years under supervision and to the satisfaction of the employing authority.
>
> (*ii*) He shall have taken an approved course of postgraduate training in clinical psychology for one year full-time followed by one additional year of full-time service, under supervision and to the satisfaction of the employing authority.
>
> (*iii*) He shall have taken an approved course of postgraduate training of at least two years full-time. It shall be at the discretion of the employing authority whether or not he shall be required to serve under supervision for a period of one year.

It is specified that supervision is to be by a Senior or Top Grade Clinical Psychologist. The period of service under Conditions *i* and *ii* may be extended by one year at the discretion of the employing authority. A clinical psychologist who qualifies in one of these ways for reduction of his period of probationary service also qualifies for a corresponding increase in his starting salary, e.g., if he qualifies under Alternative *ii* for a reduction of two years, he starts at a salary two points above the minimum, that is at £600 per annum.

The original conception of "inservice" training has therefore been undergoing modification. The consensus of opinion among psychologists is that in clinical psychology, as in other fields of applied psychology, full competence is best achieved through the combination of postgraduate academic training followed by work under supervision on the job, with this period so organised as to give experience of all the principal kinds of work carried out. This view is not confined to psychologists, but is the direction in which current developments are moving. It is also fully appreciated that the profession needs, in addition, to receive a continuous input of research experience. In fact it has. The most recent development is an explicit recognition of this need. A higher degree now qualifies an entrant to the National Health Service for a higher starting salary. The latest agreement (PTA Circular 52) specifies that one annual increment of salary may be added to the starting salary for each year of postgraduate study "in preparation for a higher degree or as a research student" up to three years. These increments do not apply to time spent in postgraduate training in clinical psychology, the allowances for which have been described above, nor is the length of probationary service affected. An entrant who possesses both postgraduate training in clinical psychology and research experience does, of course, qualify for increments of salary on both counts, up to a maximum starting salary of £615, and for a reduced period of probationary service in view of his training. The advantages of higher degrees and research experience are thus recognised, while emphasis on adequate training and professional experience is retained.

The foregoing is an account of changes in formal agreements. In examining them and the salary scales they contain, it is also relevant to take account of promotion into the higher grades. The following is one example. An able clinical psychologist, now between 35 and 40, was awarded a PhD in 1950; in 1951 he was appointed into the Senior Psychologist Grade; and in 1957 promoted into the Top Grade. There are also other recent and more numerous cases of well trained and able people under 30 who have been appointed into the Senior Psychologist Grade.

The past decade has therefore seen a rapid growth of clinical psychology in Britain from small beginnings. Throughout this time there has been no lack of interest on the part of psychologists. The expanding body of clinical psychologists has continued to be keenly aware of its own problems of training and professional service and has maintained its search for ways of improving its standard of proficiency. In 1945 a group of members working as professional psychologists in the field of mental health was formed in the British Psychological Society. This group, which has so far been

known as the society's Committee of Professional Psychologists (Mental Health), had 77 members at the end of 1945, of whom the great majority were educational psychologists working with children. It now has some 400 members, of whom over 100 are members of an Adult Section, formed in 1951, for clinical psychologists working with adult patients in the National Health Service. Subcommittees of these groups, in collaboration with others of the society's committees, are actively concerned in the formulation of policies for improving standards of qualification and proficiency and extending training facilities in the universities and elsewhere.

Psychologists in Britain have been vigorous in shaping their professional status. Training facilities are being extended and efforts made to extend them further. Higher degrees are encouraged. The structure of the psychological service is flexible, and adjustments have been made in response to new demands. Effective channels of communication have been established between those concerned with training and practice in clinical psychology, between employers and employees in the National Health Service, and between the British Psychological Society and the Ministry of Health. The past ten years have been years of progressive development, development which there is every reason to suppose will continue and increase.

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Comment

Psychology in Communist Countries Other than the USSR

Since the appearance of my two reviews of recent Soviet psychology (*Contemporary Psychol.*, 1957, **2**, 93–101 and *Science*, 1957, **126**, 1100–1107), I have received a number of inquiries about psychology in Communist countries other than the USSR, specifically whether it, too, is so thoroughly Pavlovianized? The answer—based partly upon reports in Soviet periodicals and partly upon sample readings of the other countries' original psychological literature—is by no means an unqualified "Yes." Certainly, except for Yugoslavia, Pavlovianism is being propagated with full force throughout the Communist world—through numerous translations of Soviet textbooks, special magazines devoted to the dissemination of Soviet science and culture, special Pavlov pamphlets, specially convoked Pavlovian conferences, large-scale intervisiting of Soviet and other Communist countries' psychologists and psychophysiologists, and other means and media of influence. Yet as the record stands now, unmitigated Pavlovianization of psychology, psychiatry, and related fields has seemingly been attained only in Rumania and Bulgaria, with Czechoslovakia a close second. Rumania's *Revista de Psihologie* resumed publication in 1955 as an up-to-the-hilt Pavlovian organ, and Pavlovian are also the country's other not-too-many recent psychological publications. Likewise, but with some qualifications, the two psychological periodicals in Czechoslovakia, *Československá Psychologie* in Czech, begun in 1957, and the older *Psychologický Časopis* in Slovakian abstracted by Choynowski in the *Psychological Abstracts*, are essentially Pavlovian. Bulgaria has no psychological periodical of its own and in general little specific psychological research and thought, but what has been published there—such as *Psychology in the Light of Stalin's Work on Linguistics and of Pavlov's Teachings* by Kiselevich, Head of the Department of Psychology at Sofia University (Bulgaria's only university), and *The Learning Process in the Light of Pavlov's Teachings* by Pyr'yev of the Bulgarian Academy of Sciences—differs in no way from current Russian equivalents.

On the other hand, there is sufficient evidence to think that psychologies in China and in Poland have become, under the impact of Pavlovian propaganda and the aegis of the Communist State, only much more what we would call behavioristic (the Russians will resent the American appellation), without being fully metamorphosed into a not-to-be-questioned Pavlovian orthodoxy. (It must be remembered that a strong behavioristic tradition existed in pre-Communist China and that in pre-Communist Poland Kornorski and Miller contributed very significantly to Pavlovian psychophysiology.) And—what is much more striking—psychologists in East Germany are in the main holding on to Gestaltistic orientations! I do not read Hungarian and thus know nothing of what is happening to psychology in that country, do not read Albanian but assume that not much could happen there, and would like to add that my information on Chinese psychology was gathered wholly from Russian sources. A review of original Chinese current psychological literature is very much needed.

The case of psychology in East Germany calls for special comment. As is known, that country, unlike China and Poland, is sociopolitically and ideologically very much within the assigned orbit of the Soviet Union and specifically serves as a sort of outside-Russia world-center of Pavlovian translations and propaganda. Yet Kurt Gottschaldt, the well known Gestaltist, appears by all tokens to be its leading psychologist: Editor of the *Zeitschrift fur Psychologie mit Zeitschrift fur die Angewandte Psychologie und Charakterkunde*, Head of the Department of Psychology of Berlin University, Member of the East German Academy of Sciences, and Director of the academy's Laboratory of Experimental and Applied Psychology. More than that, Hans Hibsch, the German correspondent of the Russian *Voprosy Psikhologii*, writing in its 1957 May–June issue (pp. 131–133), plainly disapproves of associationism and declares that "a true understanding of the concepts of *Gestalt* and *Ordnung* are the beginning of dialectical thinking," and then candidly adds:

> We cannot pass up silently the fact that great difficulties arose among us [German psychologists] in connection with the dogmatic and one-sided attempts to transport wholesale the teachings of Pavlov into psychology. Clearly, a science, like psychology, equipped with a whole arsenal of investigatory methods cannot content itself with a single method borrowed from physiology.

Russian psychologists and psychophysiologists have been banned and branded for incomparably lesser heresies; whereas, in this case, all the editors of the *Voprosy* seem to have done is to append a brief sentence-footnote that "with the author's evaluation of Gestalt psychology one canot agree."

Surely, the special leniency with respect to German psychology is unprecedented and surprising and could, furthermore, hardly be explained by merely pointing to the general relenting of Pavlovian orthodoxy in the last two years. For while such a relenting did occur, it, by all signs, has encompassed only freer within-the-

fold modifications and extensions of specific Pavlovian principles—such as Anokhin's novel feedback concept of inhibition—and by no means general renunciations of the doctrine and the extolling of Gestalt psychology. Another set of events needs thus to be considered: the possibility that the well indoctrinated German psychologists have somehow managed to effectively resist conversion by brimstone and fire and that for reasons of expediency the Russians have decided to make concessions to "unenlightened" but inveterate views (somewhat like their concessions to religious beliefs, one might suppose). However, it should be pointed out that such concessions are typically intended and kept up solely as temporary tactics, and that otherwise there is not a semblance of doubt that Soviet theorists are determined that there be only one psychology in the whole Communist world and that this psychology be the one of Pavlov. Whether the determinations will be fully realized is, of course, a matter of the future, depending—one should know—much more upon the general course and state of Communist ideology than upon special developments in psychology as such. "The whole dominates the destiny of the parts" is a principle of Soviet praxis—a principle, incidentally, attributed to their own philosophy, not to Gestalt psychology, and that now is held to be wholly compatible with Pavlovian associationism.

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Two Contradictory Assumptions Underlying Prognosis and Movement with Psychotherapy

Recently, there has been considerable effort expended in devising measures which will prognosticate improvement with psychotherapy and in finding measures which will measure movement with psychotherapy. To date, these efforts have been concerned with what could be called "internal" validation with some criterion of improvement. There is, as yet, little crossvalidation *between* different methods of prognostication or between methods of judging movement. As the interest in attempts to objectify these judgments increases, it is expected that crossvalidation studies will appear.

This note has relevance to the expected disagreement which may appear when various prognostic measures are compared with each other. The same remarks apply when methods of measuring movement in psychotherapy are compared with each other instead of with a criterion of improvement such as staff judgment.

In the course of thinking about our current research program concerning prognosis and measurement of movement with therapy, we found what appears to be a hidden contradiction in the assumptions underlying both judgments of prognosis and judgments of movement. Simply stated, the contradiction is this: in staff meetings when prognosis is discussed, one usually considers the patient's assets. A good prognosis is correlated positively with assets. If he has few disabling symptoms, has not been hospitalized frequently, is working, has ability to obtain self-understanding, has fair ego strength, etc., the prognosis tends to be favorable. This same thinking is apparently basic to estimates of prognosis obtained using the Rorschach Prognostic Rating Scale (3) and Feldman's modification of the MMPI (1), which we are using in our study. But a contradictory assumption seems to be operating when the MSRPP (4) and the J. McV. Hunt Movement Scale (2) are used as prognosticators. The MSRPP states: "The best single predictor of reduction in number and severity of symptoms is the morbidity score. The higher the morbidity score the greater the improvement" (4, p. 10). On the Hunt Movement Scale, the patients showing the most upward movement are those "whose opening adaptive and situational status . . . was low and whose closing status is higher" (2, p. 39). The MSRPP and the Hunt Movement Scale in effect correlate good prognosis with the amount of deficits rather than with the amount of assets.

Either approach to judgments of prognosis is tenable so long as the assumptions are understood and defined. Some confusion may be expected, however, when prognostic methods based on incompatible assumptions are compared with each other, instead of with a criterion of "improved-unimproved."

An obvious corollary of the above determines our thinking about measuring movement with psychotherapy. Sometimes we tend to think that, if a patient reaches a "normal" adjustment, undefined, he shows movement. The closer he is to the normal adjustment before therapy, the more likely he is to attain it; yet a patient far from the normal baseline can show much more *movement,* even if his end adjustment is below that of the "better" candidate for therapy.

So far we have pointed out the shifts in assumptions which take place when we think of prognosis and when we think of movement. There is one other important aspect of this shift in frame of reference. Suppose that, in making an estimate of prognosis at the beginning of psychotherapy, we base our judgment on how close the patient can come to normal adjustment. If we think the patient will be able to approach a normal adjustment, we give him a good prognosis. After therapy, we make a judgment of amount of movement. This is usually based, not on his aproach to a normal adjustment, but on how far he has moved from his pretherapy baseline or pretherapy adjustment. This is not the frame of reference used in making our prognostic judgment; we have shifted our frame of reference when we start talking of measuring movement.

It is hoped that this note will stimulate examination of *why* different methods of judging prognosis or judging movement may not correlate highly with each other, instead of permitting the conclusion that there is nothing of value in them because they do not agree with each other. We must consider whether we are basing our judgments of good prognosis on the amount of assets of the patient or on the amount of deficits of the patient.

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Clinical Psychology in Britain

Maher's article with the above title (*Amer. Psychologist*, 1957, **12**, 147–150) shows the dangers of generalizations based on inadequate data and on a lack of familiarity with the frame of reference to which the data belong. A broad picture of the development of clinical psychology in Britain is given by Summerfield (see article in this issue). Here it is intended to point out some of the fallacious inferences drawn by Maher from his limited observations.

Maher affirms that there has been an "irrational and consistent" discouragement of higher degrees (and thereby of higher status) for clinical psychologists and that the "holder of a higher degree almost invariably seeks employment other than in the hospital service. If he does enter the service, there is a high probability that he will leave for other employment." Maher gives no real data to substantiate these generalizations. The table of "Salary Rates for PhD Entrants" during the years before 1948, 1948–1953, and after 1953 is both meaningless and misleading unless Maher can provide figures of PhD entrants during these periods and a significant falling off in their numbers. The pertinent questions in this connection are: How many mental hospitals employed full-time clinical psychologists with doctoral qualifications (or any psychologists at all) before 1948? Were they employed primarily for research or for service purposes? Did they have any specific salary scale, grade structure, conditions of work, etc.? Were their positions temporary or established? One cannot speak of career prospects unless answers to these questions are available. The fact is that the field was just beginning to develop before 1948. There were few psychologists employed (it would be surprising if there were more than 15) and very few indeed with doctorates. Many were employed with expectations of research rather than service, and what the psychologist could do in service was itself in the process of development. There were varying and often indefinite conditions of salary and work, and the few psychologists with doctorates did not enter at "over £700 annually" as Maher thinks (the writer himself entered the hospital service in 1945 at a salary of £540). In such a situation it is erroneous to speak of salaries on entry. Entry to what? Similarly, Maher gives no data about PhD entrants between 1948 and 1953 nor about those with higher degrees who, wishing to work as clinical psychologists, were deterred by the salary scale since 1953. With regard to psychologists leaving the clinical field to take up more remunerative employment elsewhere, again no figures are given. Maher's illustration of a psychologist taking up "a position in industry at exactly five times his hospital salary" is an isolated instance which gives a totally false impression both about the relative salaries in the two fields and about the number of psychologists who leave the field for higher salaries. There are, in fact, very few clinical psychologists (PhDs or otherwise) who have left the field. In any case, without the necessary data how can Maher speak of "a high probability" of PhDs leaving the field? For the analogy of Gresham's Law to apply, it has to be shown that a currency exists and that the nature and number of good and bad coins are identified.

There is little evidence to show that there has been an "official structuring of roles" or that there has been a calculated policy on the part of employing authorities to discourage higher salaries and status for clinical psychologists. In a new and vigorously developing field, it is not unusual for anomalies to arise; but the consistency with which improvements have taken place despite national financial stringency shows that, contrary to a "premature crystallization of roles" and status, there has been both flexibility and good will in the profession's negotiations with the Health Ministry. The profession itself is acutely conscious of the many problems of training, service, recruitment, and status as shown by the growth and activities of the Adult Section of the Committee of Professional Psychologists (Mental Health).

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THE EVALUATION OF INSTRUMENT DISPLAYS: A POINT OF VIEW [1]

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ONE of the most common duties to fall to an applied experimental psychologist is evaluation. The sponsor may or may not specify objective evaluation. But devices, procedures, etc. must be appraised, their behavioral consequences weighed, and the applied experimentalist is very frequently expected to assume the responsibility.

This may not be the role which he sees for himself. He may prefer to think of his proper function as "research," and he may look upon evaluation as "fire-fighting," an unavoidable nuisance, something to be put up with in order to gain support for more fundamental research. The asumption seems to be made that the more fundamental research can not justify its own support, but that the opportunity for such endeavor can be acquired in exchange for services of an applied nature. These services are typically conceived as consisting of experimental studies leading to information which can be applied to some practical decision.

The University of Illinois for the past few years has operated an evaluation facility at the Wright Air Development Center. The purpose of this facility is to provide objective evaluation of instrument displays proposed for installation in the cockpits of military aircraft. The experimental psychologists manning the facility have wrestled with the problem raised above and as a result of their inner turmoil have evolved a point of view in which the conflict largely disappears. While this point of view is an adaptation to the exigencies of a display evaluation facility, it seems likely that it may have more general import. We think that this point of view may help to illuminate the problems of those who must provide an objective evaluation of other things whose worth depends at least in part upon their behavioral consequences.

THE FUNCTIONS OF INSTRUMENT DISPLAYS

The control of a machine by a man may be conceived as a process of information exchange between man and machine. This exchange has two phases: the transmission of information from man to machine by way of controls, and the transmission of information from machine to man. The latter transmission can be direct, or it can be instrumentally mediated. Certain components of the machine's output—e.g., acceleration, in the case of an aircraft—act directly upon the man's senses, while others have to be indicated in some fashion in the displays of instruments.

A display indication is a sign,[2] and like all signs it may be studied in three relational aspects. We may study it in its relation to the human operator's behavior, in its relation to the condition which it designates, and in its relation to other display indications.

The relation between a display indication and the operator's behavior is one of direction or command. The sign may direct the operator to respond in a certain way. A certain display indication, for example, may direct the operator to throttle back or to throw a switch.

In virtue of its relation to the condition it designates —its *designatum*—a display indication informs the operator of that condition. To illustrate, an indication of .5 in a vertical speed indicator designates a vertical speed of 500 feet per minute. It informs the operator that his aircraft is climbing at that rate.

In general, the operator's behavior takes account of the condition designated by the display indication in virtue of the presence of that display indication. But a display indication participates in yet a third relation. By their relations to each other, display indications can perform computations which the operator might otherwise have to perform. Two needles in a circular display illustrate display computation. Each needle is deflected through some angle from the origin of the display. These angles are display indications which designate conditions. But there is also an angle between the two needles, and this angle is also a display indication. It designates the difference between the two conditions designated by the first two angles. Thus, by their relation to each other, the first two display indications perform a subtraction.

[1] This paper is based upon material presented at the Aero Medical Laboratory's Conference on Psychological Aspects of Cockpit Design (3) at the Wright Air Development Center, October 24–25, 1956, while the authors were on the staff of the University of Illinois.

[2] In the following discussion we have drawn on the formulations of Charles W. Morris (e.g., 2).

The indications of a given instrument display may perform any or all of these three functions. A display indication designating an error which can be nulled by control action may have only a command function. The operator may be able to perform his task without knowing whether the indication designates the amount of deviation from desired pitch attitude or the logarithm of the pressure in a steam boiler. All he has to do is operate his controls in such a fashion that the error designated in the display is reduced to zero.

An example of a display indication which has primarily the function of informing the operator is an aircraft attitude indication. An angle and a distance in the display designate the bank and pitch attitude of an aircraft. But if the operator's task is to maintain a straight-and-level flight attitude, the angle and distance also designate the discrepancy between actual and desired attitude. This discrepancy is an error which can be nulled by appropriate control action. And since an error indication has a command function, this display indication both designates and directs.

An indication of desired performance can be superimposed over an indication of actual performance. The resulting sign performs all three functions. It consists of three component signs: the indications of actual and of desired performance and the relation between them. This relation designates the deviation of actual from desired performance and therefore serves a command function. Since the first two components (the indications of actual and of desired performance) are designative in function, and since by their relation to each other they perform a computation (subtraction), the sign comprising these three components can be seen to command, to inform, and to compute.

DISPLAY EVALUATION

Since it is the function of an instrument display in a man-machine system to present the human operator with signs, and since the signs may have the functions of command, designation, and computation, any evaluation of a display must be in terms of its success in presenting signs which perform these functions. (The evaluation is concerned only with how well the display's indications command responses and designate conditions, not with whether those responses should be commanded and those conditions designated. The latter question is important and relevant to the evaluation of other aspects of a man-machine system, but not to the evaluation of the display as such. For example, we consider that a "quickened display" is a misnomer, quickening being an operation upon the signal to be indicated rather than upon the display itself.) Since the result of display computation is a sign which either commands or designates, it is clear that the function of computation is subsidiary to the other two functions. The properties of an instrument display in virtue of which its indications are effective in command and designation are the properties which must be assessed in an evaluation of the display. These properties have been termed "command effectiveness" and "interpretability."

Some might argue that only command effectiveness need be assessed, that command is the critical function of a display indication and all other functions are subsidiary thereto. Implicit in this argument is the hypothesis of a constant relation between the man's behavior and his immediate environment, the cockpit. In the resulting conception of the man-machine system, the man is encapsulated in the cockpit, as Brunswik might have said. This hypothesis, like other "constancy hypotheses," represents an oversimplification of the human operator's behavior. The man's behavior we believe to be most adequately conceived as adaptive to the environment, of which the cockpit is but a part. This environment is known to the man largely through his instrument displays, whose indications he interprets. His behavior does not consist simply in the nulling of error indications as they occur, but rather takes account of the conditions designated by those indications.

Experimental Evaluation

By experimental evaluation is meant measurement. An instrument display is evaluated experimentally by measurement of its properties of command effectiveness and interpretability. These are psychological properties—i.e., properties which are described in terms of behavior. Specifically, the command effectiveness of a display is measured by a subject's ability to discriminate the commanded responses from alternative responses. And the interpretability of a display is measured by a subject's ability to discriminate the designated conditions from alternative *designata*.

The psychological properties of a display are distinguished from its physical properties—those which are discribed in terms of the physicist's language, such as shape, size, frequency, intensity, and rate. And just as physical properties are measured by physical experiment, so are the psychological properties measured by psychological experiment. Psychological experiment, then, is the method of evaluating an instrument display.

Psychological Experiment

In a psychological experiment, a subject is given a task and his response to a stimulus is measured. If the task is invariant and the characteristics of the subject are held constant either experimentally or statistically, variation in the response can be related to variation in the stimulus. Let us consider the four elements of a

psychological experiment as applied to instrument display evaluation.

The task. This is the behavior which the subject is required to produce in response to a stimulus. To evaluate the psychological properties of an instrument display, the experimenter must devise tasks such that performance will vary as a function of the subject's success in making certain discriminations.

The response. Evidence that the subject has discriminated the response commanded by a display indication can be of two kinds: the subject can indicate that response by a sign, such as a check on a multiple-choice answer sheet, or he can actually produce the comanded response itself. In the latter case, the response may be discrete, such as the throwing of a switch, or it may be continuous as the stimulus changes continuously in time. If the response is discrete, it can be described as right or wrong, or as wrong by a certain amount. A continuous response can be similarly described at any instant. Methods exist by which it is possible to measure the time during which a continuous response is right (time on target) or to integrate the instantaneous error or squared error with respect to time.

Discrimination of a designated condition can be demonstrated by designating that condition by a sign *other than the display indication.* For example: a subject may designate the altitude which is designated in a three-pointer altimeter by saying "41,550 feet." The sign "41,550 feet" is not the same as the configuration of three pointers in the display, yet both signs designate an altitude of 41,550 feet. Such discrimination measures the interpretability of an instrument display. This property is easily confused with the legibility of a display, which is measured by the subject's ability to discriminate an indication from alternative indications. He may demonstrate such discrimination by reproducing an indication or by identifying an indication among a set of alternatives. If our altimeter display were a direct reading counter, the response "41,550" would be a reproduction of the display indication. It would demonstrate that the subject had read the indication, but not that he had interpreted it. Thus a response which would measure the interpretability of one instrument display may measure merely the legibility of another.

Grether (1) has taken advantage of this fact to estimate the time required to interpret an altimeter display. He subtracted the average time required to report the altitude designated by an indication in a counter display from the average time required to report altitudes designated by indications in each of several other displays. The differences were considered to be the interpretation times of the respective displays.

The subject. In an experiment addressed to the evaluation of an instrument display, the subject is in a strict sense a measuring instrument. He is applied to the display in the same fashion as a light meter or a protractor might be applied, and his response to the display is a description of the display in the same sense as the reading yielded by a physicist's instrument. (It might be argued that a subject's response is descriptive of the subject as well as of the stimulus, but this would be true also of the reading yielded by any other instrument.)

Like any other measuring instrument, he is subject to errors of both the variable and constant varieties. And the conventional means of nullifying such errors are applicable. Variable error is reduced by application of several different instruments or by repeated application of the same instrument. The latter expedient is, of course, limited by the posibility of change in the instrument's characteristics through use, a possibility which must be allowed for in the experimental design. Constant error—in this case, the error introduced by sampling the population of subjects—is reduced by the application of several different measuring instruments. In comparative evaluations of instrument displays in which each display is measured by the same subjects, however, the subject sampling error is of minor consequence.

The stimulus. This, of course, is the instrument display indication. It is presented to the subject by the instrument display and is determined by three things: (*a*) the *designatum*, (*b*) the form of the relation between display indication and *designatum*, and (*c*) the display itself. In any experiment addressed to the comparative evaluation of two or more instrument displays, *c* would be an independent variable, while *b* should be invariant, and *a* should not vary systematically from one display to another.

The display has many properties, both psychological and physical, in addition to its command effectiveness and interpretability. Among its other psychological properties are visibility and legibility. It is clear that the interpretability and command effectiveness of a display depend upon its visibility and legibility. The difficulty of discriminating the presence of an indication from its absence and the difficulty of discriminating the indication from other possible indications are bound to be reflected in the difficulties of discriminating among possible *designata* and among possible commanded responses. But the subsidiary role of the properties of visibility and legibility is clearly evident. We increase these properties of a display in order to enhance its interpretability and command effectiveness, and not for any other reason.

An interesting possibility which may be noted here is that, in the operational situation, the discrimination of commanded response and designated condition may

be vicariously mediated. A light plane pilot is informed of his thrust, not merely by his tachometer, but also by the configuration of attitude, vertical speed, and airspeed indications, as well as by auditory cues. And the response of throttling back is commanded not merely by an excessive RPM reading, but also by a tendency to climb while maintaining a constant airspeed or a level attitude. The pilot descriminates between his thrust and other possible thrusts, and between the commanded corrective action and other possible corrective actions. The validity of these discriminations is enhanced by their vicarious mediation—by the fact that they are not dependent each upon a single channel of information with less than perfect validity, but upon a multiplicity of cues.

Generalization of Display Evaluation

The result of an experimental evaluation of an instrument display is a measurement of one or more of its psychological properties. But we are also able to measure its physical properties. The joint predication of psychological and physical properties of a display—i.e., the assertion of a relation between psychological and physical properties—is a *principle of display design*. For example: "The interpretability of an attitude indicator display is increased by substituting crosshairs for the airplane symbol." "The command effectiveness of a continuous feedback display is enhanced by the addition of an anticipatory index." In these principles, a relation is asserted such as might have been discovered in a psychological experiment—a relation between physical and psychological properties of displays.

Although each principle is demonstrated to hold only under the conditions of the experiment, their generality is clear. This degree of generality can seldom be achieved in a comparative evaluation of instrument displays which are actually under consideration for use. Usually many factors are confounded in the differences among such displays. The conclusion of such an evaluation, if indeed any conclusion can be reached, is likely to take such a form as: "Better performance was obtained with Display A than with Display B."

If the desired generality cannot be achieved with actually proposed displays, the psychologist can design displays specifically to embody certain principles. Other displays can then be designed so as to differ from the experimental displays only in certain critical respects. If the behavior mediated by the different specially designed displays is reliably different, that difference is susceptible of generalization as a principle of display design.

Of course, the question as to the relative merit of several actually proposed displays is still unanswered. But, given design principles relating each of the physical differences among these displays to its corresponding psychological difference, the relative merit of the displays can be predicted analytically—without specific experiment.

SYSTEM EVALUATION

Instrument displays have been evaluated in terms of the output of the man-machine system of which they are components. For example, the degree to which maneuvers are successfully performed in actual or simulated flight is taken as a measure of the excellence of the aircraft instrument displays being used. Variation among such measurements of system performance may indeed reflect differences among displays. But, even with the closest feasible experimental control, variation in the output of a complex man-machine system is very great. It is contributed to by all those factors which affect the operator's behavior, such as the display, as well as many more. The result is that the psychologist who seeks among measurements of system output for a difference between displays is quickly lost in a wilderness of experimental error.[3]

For practical purposes, system performance measures the excellence of the man-machine system. It provides no more than a lower limit to the excellence of any component. The display, and all other components, must have been at least good enough to permit the system to perform as well as it did. But which component, or group of components, imposed the upper limit on the system's performance we do not ordinarily know.

To evaluate an instrument display in terms of the output of a man-machine system is analogous to using the output of the man-machine system to evaluate a vacuum tube. Although a satisfactory system output would tell us that the tube is good enough for the purpose to which it is put, we would ordinarily have recourse to a tube tester instead.

Of course, the system output is the justification and final criterion for all the components. But as a basis for decisions about components we need more exact information than a description of system performance. The measurement of system performance has its role to play after a system has been assembled of analytically evaluated components. Its role is the evaluation of the system as such. Psychologists may have much to offer in such an evaluation, but it is not primarily a psychological problem. The measurement of behavior is what a psychologist is uniquely qualified to do, and such measurement is of the essence of display, not system, evaluation. For establishing display principles,

[3] Some years ago in seeking a criterion measure of bombardier proficiency one of us naively proposed the circular error of a bomb drop. His proposal was rejected on the grounds that the sortie-sortie reliability of such an index was substantially zero.

then, display evaluation can be expected to be more useful than system evaluation.

WHEN TO CONDUCT AN EVALUATION

The experimental psychologist not only conducts objective evaluations of instrument displays, but he also has the responsibility of advising his sponsor as to whether such an evaluation is worthwhile. If the sponsor needs information upon which to base a choice among displays proposed for a particular system, it is probably already too late for experimental study. It is seldom that an experimental evaluation of an instrument display can be completed in less than 6 months. And instrument displays must be specified in complete detail at least 18 months before an instrument's development is to be completed. After all elements of a weapon system have been completely developed, it takes at least another 18 months before the first model can be produced. So an experimental problem must be formulated at least 3½ years before a weapon system is to be delivered if its solution is to affect the design of that system. And the enormous cost of a retrofit program makes it unlikely that experimental evaluation will result in any modification of an existing system.

We conclude from the foregoing that, when information is needed upon which to base a design decision, it must already be in existence to be effective. What is really needed at the design stage of a man-machine system is inventiveness and ingenuity in applying established display principles and existing understanding of man-machine relations. The psychologist should prepare himself for this contingency by accumulating such understanding. But how is he to foresee the need if, by the time concrete design information is needed, it is already too late to get it? There is only one answer: the psychologist must pursue principles of display design which are of such a general nature that they will be applicable within a broad domain of design problems.

Those who insist that no design decisions be taken unless based upon directly applicable experimental findings can only hinder the process of development. If the necessary evidence is not available when the decision must be made, the decision must be made as well as possible in the absence of that evidence. This is not a novel procedure. Design has always proceeded on the basis of best guesses, sound evidence being used wherever possible.

Nor should the psychologist turn in frustration from the problem and undertake the solution of some other design problem anticipated for the near future. If his research is to be effective, it must be addressed to enduring problems which will be encountered again and again in the foreseeable future. As for the problem at hand, we hold that he should forthrightly advise his sponsor that experimental evaluation is not feasible. When a hard analysis shows specific research to be pointless research, then the psychologist's ethics should prevent him from taking money to perform it.

THE POINT OF VIEW

The point of view which we have set out to present rests on six major principles:

1. The indications of an instrument display can perform the functions of command, designation, and computation.
2. An evaluation of an instrument display must be in terms of its command effectiveness and its interpretability.
3. These properties are measured by psychological experiment.
4. The result of such an experiment can be generalized as a principle of display design.
5. Measurement of system output, instead of the operator's behavior, is the basis for system evaluation, not display evaluation.
6. Experimental evaluations of concrete displays can not ordinarily affect the design of the system for which those displays are proposed.

Our point of view can be stated simply in this way: the only research which can in any significant measure lead to applied decisions is research of general import. This kind of research, therefore, justifies its own support and is indeed the only kind which can justify any support.

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Psychology in the News

Social Notes, White House Division . . .

At the famous White House science dinner, psychology was represented by S. S. Stevens, Director of the Psycho-Acoustical Laboratory of Harvard University.

Another Prize Winner . . .

Irving A. Taylor, Assistant Professor of Psychology at the Pratt Institute in Brooklyn, New York, was awarded the 1957 Sociopsychological Prize of $1,000 at the winter meeting of the American Association for the Advancement of Science. His subject was "Similarities in the Structure of Extreme Social Attitudes."

He is 32 years old, received his MA at the University of Houston, his PhD at New York University, and told the *APA Directory* his interests were symbolic processes of language and thought, perception, art and creativity.

We are still waiting for that study, which urgently needs to be done, of the correlations between what people list as interests in the *APA Directory* and what they actually do, and furthermore excel in.

From The New Yorker . . .

The following item is reprinted in its entirety from a magazine published in New York:

Dr. Albert S. Thompson, professor of psychology, Teachers College, Columbia University, will speak on "Human Relations in the Choir Loft" during a meeting of the Northern Valley Chapter, American Guild of Organists.—News item received by the *Bergen* (N. J.) *Record*.

And the *New Yorker:* "In a word, look straight at the music."

Thanks to Lear, Far or Near . . .

From across the Atlantic comes an editorial article commenting on Vice-President Richard M. Nixon's recent interest in the "social sciences." A new British magazine, *The New Scientist*, appeared last year. It comes out every week and in tone is somewhat between *Scientific American* and the AAAS journal, *Science*. Its American correspondent writes, in part:

No man who can sense what the people are thinking would be making the commitments Nixon has made during the last few weeks if he did not feel the country preparing itself to consider an American position in the world based less exclusively on armed force and relatively more on constructive peaceful measures to build friendship.

. . . in spite of decades in which the Federal Government has been the largest consumer of social science skills, in spite of the obvious inability of any major Government Department to function without them, the social sciences have been the abandoned children of official science policy ever since Civil War observation balloons lofted technological science high in Lincoln's mind.

The significance of Vice-President Nixon's attitude is in his timing. He did not speak up before, when the Senate Democrats spoke. He is taking a stand now, reflecting what he believes public opinion to be, in advance of the President's nominations, due on 10 May, for eight of the twenty-four seats on the National Science Foundation's governing board. By proportionate count of scientific noses, the social sciences would be entitled to five or perhaps six of the twenty-four votes. Right now they have two. America's potential for constructive power is in that degree weakened.

Space Filler . . .

Newsweek magazine has inaugurated a department called "Space and the Atom," and perhaps that will end a perennial difference we have had with its editors as to whether psychological news belongs under "Science," as we suggest, or under "Medicine," as they frequently decide.

Anyway, we may follow their lead, for our spacemen continue to expand our universe. Now B. Richard Bugelski, Professor at the University of Buffalo, is advising the Cornell Aeronautical Laboratory on measuring intellectual reactions of monkeys under conditions of weightlessness. The magazines are carrying photographs of the key equipment and the subject: a monkey facing two levers which he will move on seeing a red or a green light. The whole push would be sent up by rocket, and the animal's reactions recorded by telemetering devices and motion pictures.

It all sounds interesting, but there is something quite disturbing and haunting about the photograph of the monkey, hands gripping the levers. One can't exactly pin point the disquieting element . . . perhaps there is a haunting resemblance to some other well-loved animal . . . his brain conditioned to past experience, calmly pulling levers while his controlling environment moves at rocket speed out of a world he never understood, into another world requiring new understanding.

—M. Amrine

Psychology in the States

The State Association and the "Critical Incident"

To the APA Committee on Public Relations, it has seemed that its task of drafting a public relations policy for the association might be approached in at least three ways: deductively, inductively, and for want of a better word, imitatively. The latter would amount to taking leaves from the books of several other professions, most of them with considerable experience in the business of presenting themselves to the community and its citizens.

With due respect to the intent of these other groups, a review of some of their public information policies indicates that, at best, many of the latter are not relevant to our aims (and that, at worst, some of them are very much of the Madison Avenue school). More fitting, then, has seemed the method of working from above down—from APA's set of values to their implementation—or from below upward—from the situations in which psychologists find themselves to an abstraction of the kinds of issues and principles which seem to be involved. Using both of these approaches, the Committee on Public Relations has requested the help of state associations in compiling a file of situations characterizing present involvements. Some typical "critical incidents," reported to date by various state associations, are:

State A: A riot occurs at a state penitentiary. In the prisoners' demands that an investigating committee be formed is a specific request that such a committee include a psychologist. When the committee is created by the governor, its membership includes a psychiatrist, but no psychologist. The state association debates what, if anything, should be done.

State B: A nonprofit organization announces that it will hold classes in hypnosis, open to the general public. Members of the state association seek the cooperation of other professional groups in pointing out the potential harm which might result. As a consequence, the proposed classes are canceled.

State C: A large city night school has been pressured by some members of the board of directors to drop its mental health talks. The principal of the school contacts the state association, requesting that it send a letter in support of the continuation of mental health lectures.

State D: A reputable psychiatrist has on his staff a person whom he has allowed to be designated a "clinical psychologist." The latter does not meet the commonly accepted standards implied by the title. Discussion of the situation with the psychiatrist yields, at best, unsatisfactory results. The state association ponders what, if any, further approaches should be made to other representatives of the medical profession concerning the situation.

State E: A physician with relatively little psychological training has been administering projective tests. He applies to the state association for membership.

While not necessarily earth-shaking in their implications, the above incidents are representative of the everyday considerations to be faced as state associations become more active and psychology becomes more visible. A comprehensive file of such involvements—both major and minor—might well be a pillar on which a realistic public information policy could rest.

* * *

Big Brother Is Watching You. Resolved that misrepresentation is better corrected than censured, the Public Information Committee of the New York State Psychological Association is proceeding accordingly. The NYSPA Board of Directors has allotted a sum sufficient to pay for subscriptions to a representative sample of the major magazines. Designated members have assumed responsibility for monitoring each of perhaps ten publications, each one's assignment being to skim his particular magazine regularly for articles bearing on psychology and psychologists. When an appropriate article appears, the responsible person will perform a twofold function: write to the editor and the author involved; get several others to write, as individuals, expressing similar sentiments.

The objective basically is that of establishing channels of communication, conveying the notion that psychologists are attentive and responsive to what appears in the popular press. The intent is primarily that of serving constructively as consultants rather than destructively as critics. Hopefully, the ensuing liaison will cultivate in authors and editors the habit of seeking out knowledgeable psychologists as advisors.

The Trend Toward Executive Secretaries. In line with the suggestion recently made by the Committee on the Organization and Functioning of

State Associations, the post of Executive Secretary is becoming increasingly popular. Ernest Beier, President of the Utah Psychological Association, announces the appointment of Jules Van Praag as UPA Executive Secretary. Similarly, the Ontario Psychological Association has elected Ray G. Berry as OPA Executive Secretary for a term of three years.

Twin Billing. Whether dictated by reasons of economy or broadmindedness, interstate meetings are increasingly coming into vogue. The Oregon Psychological Association will play host to the Washington Psychological Association at its spring meeting on May 9–10 at the University of Portland. Similarly, barring a clash between Sputnik VII and Explorer V, the Florida Psychological Association intends to offer its hospitality to the Southeastern Psychological Association in 1959.

BPA, CSPA, and the Regional Meetings. As previously announced, four of CSPA's committees are serving jointly as committees of the Board of Professional Affairs. The forthcoming regional meetings will symbolize the union, inasmuch as members of both groups will help chair the respective meetings of state association officers.

Central in the arrangements is the role of the Committee on the Organization and Functioning of State Associations. The latter has outlined as its project of the year the preparation of a handbook for state association officers. Hence, the proceedings of the regional workshops will serve as special grist for the committee's mill. Needless to say, it is hoped that the response will equal, if not surpass, the enthusiasm displayed at last year's trial run.

Governors and Psychologists. Some time ago, the Governor of Iowa appointed a committee to study mental health conditions in the state and to make recommendations which might eventuate in legislation. At the suggestion of Charles F. Haner, Public Information Chairman, members of the Executive Council of the Iowa Psychological Association wrote letters to the Governor, informing him of IPA's interests and its willingness to serve as consultant. As a result, two members of IPA were appointed to the committee. In addition, the Governor asked that IPA appoint a representative to the Professional Advisory Committee to the Director of Mental Institutions. Leonard D. Goodstein is now serving in this capacity.

Predigestion. Whoever has had qualms about journals which remain in their wrappers will appreciate the guilt feelings of the newsletter editor who, having initiated an exchange with other state associations, finds no time to read the efforts of his fellow editors. The Maine Psychological Association has solved the problem by the appointment of an Associate Editor. The latter's function consists, not only of reading and digesting newsletters received, but also of writing appropriate resumés for inclusion in the *MPA Newsletter*. The resulting section—"News from Other States"—may strike other associations as worthy of emulation.

* * *

Repeat Announcement. Not infrequently, items intended for this section arrive by virtue of the most devious rerouting. Since the straight line is still the shortest distance between two points, interested parties are once again informed that news of interest should be sent directly to either (and preferably both) of the following:

Wilbert J. McKeachie
Department of Psychology
University of Michigan
Ann Arbor, Michigan

Erasmus L. Hoch
American Psychological Association
1333 Sixteenth Street, N.W.
Washington 6, D. C.

* * *

State Association Officers. The following rosters of officers have been announced:

District of Columbia Psychological Association

President: Joan H. Criswell
President-elect: Max Meenes
Secretary: Bertha P. Harper
Treasurer: Maurice Lorr

Psychological Association of Western New York

President: Marvin J. Feldman
President-elect: Thomas R. Sonne
Secretary: Sue A. Gruen
Treasurer: Eleanor A. Jacobs

State Convention Calendar

Utah Psychological Association: April 19, 1958; Provo, Utah

For information, write to:
Jules Van Praag
Utah State Hospital
Provo, Utah

—W. J. McKEACHIE
E. L. HOCH

Psychological Notes and News

The Sixth National Conference of the United States National Commission for UNESCO. Well over one thousand delegates from various scientific organizations convened on November 6–9, 1957 in San Francisco for the conference. Donald G. Marquis, a member of the commission, Henry Clay Lindgren, and K. S. Yum represented the APA. The purpose of the conference was to promote mutual understanding and cooperation between Asian nations and the United States. Christian Herter, Under Secretary of State, gave the keynote address. Work groups covered wide areas of science, art, religion, literature, government, and public affairs. The theme of the entire conference led to the conclusion that our understanding of Asian peoples is most urgent. Success depends largely upon our knowledge of their cultural backgrounds, their aspirations, and their tensions. Among scientists, the psychologist has a unique responsibility and a privilege in the promotion of such significant human relations. Serious consideration of a new curriculum is needed in training the young psychologist in this area.

Delos D. Wickens, who has served as Associate Editor of the *Journal of Experimental Psychology* during the processing of the manuscripts received in 1956 and 1957, has resigned this position but will continue to serve the journal in the role of Consulting Editor. **William K. Estes** has accepted appointment as Associate Editor, effective April 1, 1958.

The American Board of Examiners in Professional Psychology, Inc. expresses its appreciation for the services of its Diplomates who served as readers of essay questions of candidates taking its ninth Written Examination in November 1957:

Dorothy V. Anderson
Donald E. Baier
Joseph E. Barmack
Irwin A. Berg
Paul Bergman
Sidney W. Bijou
Douglas D. Blocksma
Milton L. Blum
Katherine P. Bradway
Joseph E. Brewer
Arnold H. Buss
Bettye M. Caldwell
Robert C. Challman
Henry P. David
Gordon F. Derner
Austin M. DesLauriers
Lewis E. Drake
Donald Ehrman
Albert Ellis
Edmond F. Erwin
Norman L. Farberow
Erika Fromm
Sol L. Garfield
Norman Garmezy
Ann M. Garner
Ralph W. Heine
Leo A. Hellmer
Ernest A. Hirsch
Milton W. Horowitz
Leonard Horwitz
Joseph McV. Hunt
Max Hutt
Walter F. Johnson
Marvin W. Kahn
Raymond A. Katzell
Bill L. Kell
E. Lowell Kelly
George A. Kelly
Isabelle V. Kendig
Sheldon J. Korchin
Samuel B. Kutash
Wallace V. Lockwood
Robert L. McFarland
John W. Macmillan
Joseph D. Matarazzo
Martin Mayman
Ivan N. Mensh
Robert S. Morrow
O. Hobart Mowrer
C. Roger Myers
Julian H. Pathman
Harold B. Pepinsky
Frances C. Perce
Edward T. Raney
Gilbert J. Rich
Alexander C. Rosen
John M. Rothney
Salvatore Russo
R. Nevitt Sanford
Stewart B. Shapiro
Edward Joseph Shoben, Jr.
Marie P. Skodak
Donald E. Super
Dik W. Twedt
Howard R. White
Harold Wilensky
Herbert Zucker
John V. Zuckerman

A major in psychology is the only requirement for admission to graduate study in psychology at the **University of California, Berkeley.** The 36-credit program listed on page 627 of the October 1958 *American Psychologist* is our own major program, including the undergraduate language requirement of the university. Major programs of other universities satisfy the admission requirement completely.

Paul F. Finner, of Tallahassee, Florida, represented the APA at the inauguration of Robert Manning Strozier as President of the Florida State University on February 21, 1958.

William Seeman, of the University of Oklahoma School of Medicine, represented the APA at the inauguration of Jack Stauffer Wilkes as President of Oklahoma City University on March 6, 1958.

Ole N. deWeerdt, of Beloit, Wisconsin, died in February 1958.

C. Frederick Hansen, of Fredericksburg, Virginia, died February 19, 1958.

Herbert S. Langfeld, a past President of the APA, died February 25, 1958.

Rosemary Lippitt, of Los Altos, California, died January 30, 1958.

Henry Angelino, Associate Professor in the School of Education, Oklahoma University, Nor-

man, Oklahoma, has been appointed to the Editorial Board of *Education & Psychology*, a research quarterly published in India. He will represent the journal in the United States. The journal is offered at a special rate to members of APA; address all correspondence to Henry Angelino.

Gertrude Baker has accepted appointment as Chief Psychologist at the Westwood Community Methodist Church Mental Health Clinic, Los Angeles, California. **J. F. T. Bugental** resigned as Chief Psychologist but will continue with the clinic as consultant in clinical psychology.

Ruth C. Boyle, formerly Supervisor of Special Services in the Irvington School System has been appointed Professor at the New Jersey State Teachers College and director of a research project on mentally retarded adolescents for the United States Office of Health, Education and Welfare.

Louis J. Cantoni, Associate Professor at Wayne State University, has been elected President of the newly formed Detroit Chapter of the Michigan Rehabilitation Association. The Detroit Chapter replaces the former Detroit Council for the Handicapped.

Arthur H. Davison has transferred from the Fresno Veterans Administration Hospital to the staff of the United States Naval Hospital, Oakland, California.

S. Norman Feingold, formerly Executive Director of the Boston Jewish Vocational Service has been appointed National Director of the B'nai B'rith Vocational Service in Washington, D. C.

Harold Kenneth Fink, formerly at the Religio-Psychiatric Clinic, has accepted the position of Director of the new Marriage Counseling Service at the Palm Beach County Guidance Center, West Palm Beach, Florida.

John F. Franklin, formerly at the Wayne County Clinic for Child Study, has accepted a position as Clinical Psychologist for the State Board of Health in Florida, working for the Collier County Health Department.

Philip Himelstein, formerly at the Air Force Personnel and Training Research Center, has been appointed Assistant Professor in the Department of Psychology at the University of Arkansas.

Thomas A. Hussman, Jr. will direct the activities of the branch office recently opened in Encino, California, by the Psychological Research Associates, Inc. **George A. Peters, Jr.** has joined the staff of the Encino office as Research Associate.

Saul C. Kupferman was appointed OVR Project Director and Senior Psychologist, and **Richard E. Rush** was appointed Counselor Psychologist, at the United Cerebral Palsy Association of Los Angeles County.

Henry B. Loess, currently at Lake Forest College, has been appointed Professor of Psychology and Head of the Department at the College of Wooster, Wooster, Ohio, beginning September 1, 1958.

Henry Lowenheim, formerly Clinical Psychologist at Kings County Hospital, has joined the staff of the Psychological Service Center, Teaneck, New Jersey. **Amedeo G. Sferra** is Consultant in Psychology and Guidance.

William McGehee, Director of Personnel Research and Training, Fieldcrest Mills, has accepted an appointment as Lecturer in Psychology, and **Adrian Solomon** has been appointed Assistant Professor of Psychology, at the Woman's College of the University of North Carolina.

Frank C. J. McGurk, Associate Professor of Psychology at Villanova University, will participate in an International Geophysical Year project concerning man in his physical environment.

James F. Magary, formerly Psychological Coordinator for the Devereux Foundation, has been employed by the Panama Canal Company as their first psychologist. He will be engaged in the clinical evaluation of handicapped children living in the Canal Zone.

Cyril R. Mill, formerly at the Allentown State Hospital, is now Director of Psychological Services, Department of Mental Hygiene and Hospitals, Richmond, Virginia.

The National Academy of Sciences-National Research Council announces the appointment of **Neal E. Miller,** James Rowland Angell Professor of Psychology at Yale University, as Chairman of the Division of Anthropology and Psychology, effective July 1, 1958.

H. H. Remmers has been appointed Visiting Professor of Psychology at the University of Puerto Rico for the coming summer session to conduct a seminar on Measurement and Evaluation at the University Level.

George B. Simon, formerly at the Air Force Personnel and Training Research Center, is now Chief of Engineering Psychology at Link Aviation, Inc., Binghamton, New York.

Andrew H. Souerwine, formerly at Trinity College, has accepted a position as Staff Psychologist for the Travelers Insurance Company, Hartford, Connecticut.

Alan D. Swain has joined the staff of Dunlap and Associates, Inc., Stamford, Connecticut. He was formerly associated with the American Institute for Research.

The following personnel changes have occurred in Psychology Services, Department of Medicine and Surgery, Veterans Administration:

Albert D. Annis has been appointed to the Clinical Psychology Staff, VA Hospital, Salisbury, North Carolina.

Richard S. Ball has resigned from the Iowa City VA Hospital to accept a position at the National Institute of Mental Health.

Ernest J. Blache, a garduate of the VA Psychology Training Program, University of Pittsburgh, has been appointed to the staff of the Psychology Service, VA Hospital (Leech Farm Road), Pittsburgh, Pennsylvania.

William A. Brett has transferred from the Biloxi VA Center to the Vocational Counseling Staff, VA Hospital, Chillicothe, Ohio.

Edwin Cohen has been appointed to the position of Chief Clinical Psychologist, VA Hospital, Iowa City, Iowa.

Max Cooper has resigned from the position of Chief Clinical Psychologist, VA Hospital, Northport, Long Island, New York.

Ernest A. Kurtz has transferred from the North Little Rock VA Hospital to the Clinical Psychology Staff, VA Hospital, Jefferson Barracks, Missouri.

Charles Leiman has resigned from the position of Clinical Psychologist, VA Mental Hygiene Cliniic, Baltimore, Maryland.

Jacob S. Nakshian has transferred from the Albany VA Hospital to the Mental Hygiene Clinic, VA Office, Worcester, Massachusetts.

Eli Rubinstein has resigned from the Washington Veterans Benefits Office to accept a position at the National Institute of Mental Health.

Joseph Southworth has resigned from the position of Clinical Psychologist, VA Hospital, Albany, New York.

Through the generosity of an anonymous donor, the **American Association for the Advancement of Science** offers an annual prize of $1,000 for a meritorious essay in sociopsychological inquiry. The contribution should further the comprehension of the psychological-social-cultural behavior of human beings—the relationships of these hyphenated words being an essential part of the inquiry. The prize is offered to encourage studies and analyses of social behavior based on explicitly stated assumptions or postulates which lead to experimentally verifiable conclusions or deductions. For further information, write to: Dael Wolfle, Executive Officer; American Association for the Advancement of Science; 1515 Massachusetts Ave., N.W.; Washington 5, D. C.

Previous winners of this prize have been: **Arnold M. Rose,** "A Theory of Social Organization and Disorganization"; **Yehudi A. Cohen,** "Food and Its Vicissitudes: A Cross-Cultural Study of Sharing and Nonsharing in Sixty Folk Societies"; **Herbert C. Kelman,** "Compliance, Identification, and Internalization: A Theoretical and Experimental Approach to the Study of Social Influence"; and **Irving A. Taylor,** "Similarities in the Structure of Extreme Social Attitudes."

Norman A. Polansky, of the School of Applied Social Sciences, Western Reserve University, has received a grant for four years from the National Institute of Mental Health for a study of the "**Influences of Social Climate on Accessibility to Treatment.**" Erwin S. Weiss has been appointed Project Director.

The Research Center of the Graduate School of Public Administration and Social Service at New York University, having completed its first study—"The Adjustment of the Severely Retarded Adult in the Community"—is presently engaged in a study of the factors determining **institutionalization of the retarded.**

The Postgraduate Center for Psychotherapy has obtained financial support for an extended trial of the feasibility of an informal research exchange and newsletter. The plan for the **International Research Newsletter in Mental Health** calls for printing what workers in mental health want to throw before their fellows for discussion, evaluation, and general information. For further information, write to: *The Newsletter;* Postgraduate Center; 218 East 70 Street; New York, New York.

The **National Institute of Mental Health** has awarded the following research grants:

Clemens E. Benda and **Edward V. Jones, Jr.**, Walter E. Fernald State School, Waverly, Massachusetts, "Personality Factors in Mental Retardation."

Charles E. Bowerman, University of North Carolina, "Parent and Peer Orientations of Adolescents."

B. Richard Bugelski and **Herbert C. Lansdell,** University of Buffalo, "Conflict in a Double Discrimination Situation."

Ernest W. Burgess, University of Chicago, "A Comparative Study of Divorced and Married Couples."

Alfred J. Butler and **Marjorie S. Mertens,** Laurelton State Village, Pennsylvania, "Self-attitudes, Adjustment, and Learning in Retardates."

Helen B. Carlson, Northwestern University, "Nonpsychotic Confusional States in College Students."

Michael R. D'Amato, New York University, "Early Experience and Adult Behavior Organization."

Anthony Davids, Emma Pendleton Bradley Home, Riverside, Rhode Island, "Study of Hospitalized Emotionally Disturbed Children."

Josi M. R. Delgado, Yale University, "Chronic Stimulation of the Brain in Monkeys."

P. B. Dews and **W. H. Morse,** Harvard University, "Basic Types of Effects of Drugs on Behavior."

Paul J. Hoffman, University of Oregon, "The Paramorphic Representation of Human Judgment."

Joe Kamiya, University of Chicago, "Behavioral and Physiological Concomitants of Dreaming."

Leon H. Levy, Indiana University, "Studies of Anticipatory Processes."

Wallace R. McAllister, Syracuse University, "Systematic Investigation of Second-Order Conditioning."

Donald R. Meyer, Ohio State University, "Studies in Primate Learning and Perception."

Charles E. Osgood, Social Science Research Council, New York, "Interdisciplinary Research Seminar on Aphasia."

Curtis Tuttle Prout and **Mary Alice White,** New York Hospital-Westchester Division, "Drug Effects in Psychiatric Research."

Irvin Rock, New School for Social Research, New York City, "Trace Mechanisms in Learning and Memory."

David M. Schneider and **L. Bryce Boyer,** University of California, "Explorations of Anthropopsychoanalytic Field Methods."

B. F. Skinner, Harvard University, "Analysis of Complex Behavioral Processes."

Fred L. Strodbeck and **Rita M. James,** University of Chicago, "Juror Reactions to Legal Definitions of Insanity."

S. A. Szurek and **Dorothy Bomberg,** Langley Porter Clinic, San Francisco, "A Statistical Study of Autistic Patients."

Janet A. Taylor and **Brendan A. Maher,** Northwestern University, "Learning, Emotionality, and Early Experience in Rats."

Robert D. Vinter and **Morris Janowitz,** University of Michigan, "Social Organization and Institutional Treatment."

J. M. Warren and **Ronald Grant,** Stanford University, "Unilateral Cortical Lesions and Learning in Monkeys."

Edward L. Wike, University of Kansas, "Irregular Training and Permanence of Learning."

J. Richard Wittenborn, Rutgers University, "A Behavior Rating Scale Procedure."

The National Institute of Mental Health has made a grant of $20,125 to establish in the American Public Health Association (1790 Broadway; New York 19, New York) a Committee on Public Health and the Behavioral Sciences to explore the area of common interest and activity between **social science and public health.**

The National Institute of Mental Health has awarded a Mental Health Project Grant to investigate the merits of a 15-week **Workshop in Self-Understanding** as a short-term form of help for outpatients on waiting lists for psychotherapy, as a preparatory experience for psychotherapy, and as a means of preventive community education. As one of its functions, the project will compile film guides to be used in conjunction with the following films: "This Charming Couple," "Unconscious Motivation," "Learning to Understand Children," "Feeling of Rejection," "Overdependency," "Feeling of Hostility," "A Family Affair," "Fears of Childhood," "The Lonely Night," "Angry Boy." *We would appreciate receiving lists of questions which others have found useful in stimulating group discussions on the above films; also descriptions of group experiments, procedures, or demonstrations designed to stimulate self-insight.* In return for such contributions, we will send a summary of group methods that we have developed to date. All materials should be addressed to: Daniel Malamud; Mental Health Education Project; State University of New York; 450 Clarkson Avenue; Brooklyn 3, New York.

"Table for Obtaining Phi Coefficients" (*Psychometrika,* 1947, 12, 17–29) has again been reprinted; free copies are available from the author: Clifford E. Jurgensen; Minneapolis Gas Company; Minneapolis 2, Minnesota.

A One-Hand Manual Language, known as the **"Hand Talking Chart,"** has been developed as a means of communication for hemiplegics. For gratis copies of the chart, write to: Hamilton Cameron; 601 West 110 Street; New York 25, New York.

Discussions of mental health aspects of advances in technology, including peaceful use of atomic energy, highlighted the thirty-fifth Annual Meeting of the **American Orthopsychiatric Association** in New York City on March 6–8, 1958.

The Psychological Department and the local chapter of **Psi Chi** at Bowling Green State University jointly sponsor a colloquium each year to encourage undergraduate and first-year graduate students in psychology to do and report research. The fourth of these colloquia was held on March 8, 1958.

James G. Ranck delivered the keynote address at the quadrennial **Convocation of Urban Life** of the Methodist Church on February 18, 1958: "The Changing City and What It Does to People: A Challenge to Methodism."

The topic, **"Controversy in Teacher Education,"** was discussed by Stephen M. Corey and Paul Woodring in a meeting at Teachers College on March 10, 1958.

A Conference on Research in Psychotherapy, sponsored by the APA Division of Clinical Psychology and financed by a $13,685 research grant from the National Institute of Mental Health, will be held in Washington, D. C., on April 10–12, 1958. The conference will bring together a representative sample of 30 research workers in various disciplines for presentation of papers and for discussion of research on effects of psychotherapy. It is hoped that the publication of the proceedings will serve to extend communication to the equally competent and experienced scientists now working in this area who could not be invited.

The Institute for the Crippled and Disabled announces the fifth work-conference on **"Developing Concepts in Community Rehabilitation Services"** to be held June 2–20, 1958 in New York City. For further information, write to: Marguerite Abbott; 400 First Avenue; New York 10, New York.

The eleventh Annual Meeting of the **Gerontological Society, Inc.** will be held on November 6–8, 1958 in Philadelphia. For further information, write to: Warren Andrew; Bowman Gray School of Medicine; Winston-Salem, North Carolina; *or* to: Joseph T. Freeman; 1530 Locust Street; Philadelphia 2, Pennsylvania.

An institute on administration and interpretation of the **Kahn Test of Symbol Arrangement** will be held in Wiesbaden, Germany, July 7–18, 1958. For further information, write to: Robert B. Ammons; Department of Psychology, Montana State University; Missoula, Montana.

The Survey Research Center of the University of Michigan will hold its eleventh annual summer institute on **Survey Research Techniques** on July 21–August 16, 1958. For further information, write to: Survey Research Center; University of Michigan; Ann Arbor, Michigan.

Clark University announces the establishment of an **Institute of Human Development.** The aims of the institute are to integrate various research programs dealing with problems of development, to train research workers in the comparative-developmental approach to behavior, and to bring to Clark scholars and teachers from related disciplines to conduct interdisciplinary seminars and research.

As a result of the recent reorganization of the personnel and training research program within the Air Force, the **Psychology Branch of the Aero Medical Laboratory** at the Wright Air Development Center, Dayton, Ohio, has assumed additional responsibilities. The revised program includes research and applied work in all areas of human engineering or engineering psychology, in training, proficiency measurement, and in forecasting the personnel and training requirements of complex new systems and equipment.

A Symposium on Sensory Deprivation will take place in Boston on June 20–21, 1958. The symposium will be a closed working meeting of invited speakers and discussants, the proceedings of which will be published.

Workshops on research in **religious and character education** will be held during June and July 1958 at Union College and at Michigan State University. For further information, write to: Character Research Project; Union College; Schenectady 8, New York.

The College of St. Catherine (St. Paul, Minnesota) has received a special training grant of $8,600 from the National Institute of Mental Health to set up a **training institute in mental health** for the administrators of colleges, hospitals, schools, and charitable institutions. The main feature of the institute will be a summer workshop, August 1–10, 1958.

The Counseling Center of the University of Chicago will offer a workshop on **"A Therapeutic Approach to Individuals and Groups,"** June 23–July 4, 1958. For further information, write to: Martha Roess; Counseling Center Workshop, University of Chicago; 5737 Drexel; Chicago 37, Illinois.

Convention Calendar

American Psychological Association: August 28–September 3, 1958; Washington, D. C.

For information, write to:
Roderick H. Bare
American Psychological Association
1333 Sixteenth Street, N.W.
Washington 6, D. C.

Western Psychological Association: April 24–26, 1958; Monterey, California

For information, write to:
Francis H. Palmer
U. S. Army Leadership Human Research Unit
P. O. Box 787
Presidio of Monterey, California

Southeastern Psychological Association: April 27–29, 1958; Atlanta, Georgia

For information, write to:
M. C. Langhorne
Box 2
Emory University, Georgia

Midwestern Psychological Association: May 1–3, 1958; Detroit, Michigan

For information, write to:
Donald W. Fiske, Secretary-Treasurer
Department of Psychology
University of Chicago
Chicago 37, Illinois

Rocky Mountain Psychological Association: May 8–10, 1958; Santa Fe, New Mexico

For information, write to:
William H. Brown
Department of Psychiatry
University of Utah College of Medicine
156 Westminster Avenue
Salt Lake City 15, Utah

World Federation for Mental Health: August 24–29, 1958; Vienna, Austria

For information, write to:
Secretary-General
World Federation for Mental Health
19 Manchester Street
London, W.1, England

A calendar of international conventions was given in the January 1958 issue of the *American Psychologist*

Bind your copies of APA journals

Arrangements have been made by the American Psychological Association for subscribers to have their journals bound into distinctively designed books. The binding is washable buckram with your name imprinted on the cover.

Prices and colors are listed below:

American Psychologist	turquoise	$4.15
Contemporary Psychology	vermilion	4.15
Journal of Abnormal and Social Psychology	dark green	3.95
Journal of Applied Psychology	slate	3.95
Journal of Comparative and Physiological Psychology	adobe	3.95
Journal of Consulting Psychology	russet	3.95
Journal of Educational Psychology	black	3.95
Journal of Experimental Psychology	red	3.95
Psychological Abstracts	green	4.15
Psychological Bulletin	navy	3.95
Psychological Monographs	cocoa	3.95
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Ship journals parcel post. Within thirty days after receipt, bound volumes will be shipped prepaid anywhere in the U. S. A. Full remittance must accompany order.

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GENERAL ARRANGEMENTS

SIXTY-SIXTH ANNUAL CONVENTION OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION

Washington, D. C., August 28–September 3, 1958

SHERMAN ROSS, *Convention Manager*

OUR Annual Convention involves two major efforts: program planning and convention administration. The APA Convention Program Committee and the divisional program committees develop the programs. The Convention Arrangements Committees provide the facilities for implementing them. Information important for program planning has already appeared in the "Call for Papers and Symposia" in the January issue of the *American Psychologist*. A proposed block-time schedule of the program will be published in a subsequent issue.

This announcement covers convention arrangements which are of general interest to the membership.

Time and Place of Meetings

The Annual Convention will be held Thursday, August 28, through Wednesday, September 3, 1958 in Washington, D. C. Most of the meetings will be held in the Statler and the Mayflower hotels. Consideration is being given to the use of Constitution Hall for some of the APA Day activities.

Publication of Convention Program

This year the complete convention program, with abstracts, will be printed in the July *American Psychologist*.

A *Convention Guide* (formerly called the Condensed Program) will be distributed at the convention Registration Desk. Members who do not register will be charged a fee of $1.00 for the *Convention Guide*.

Hotel Reservations

The Hotel Reservation form is printed at the end of these notes. The likelihood of obtaining a room of your choice in either of the headquarters hotels depends upon how early your reservation is made. Additional hotel facilities will be available through the APA Housing Bureau and will be announced in subsequent issues.

Please reserve your room well in advance of the meeting if possible. No matter when you make your reservation, however, it is very important to the APA that you identify yourself as a member or guest of the APA convention. The number of reservations made by APA members and their guests affects the facilities that hotels can justifiably make available to this convention and future ones.

Send your reservation to: APA Housing; Washington Convention and Visitors Bureau; 1616 K Street, N.W.; Washington 6, D. C.

Convention Arrangements Committees

APA members interested in matters handled by the various committees are urged to communicate with the appropriate chairmen directly. The chairmen are interested in your suggestions. On matters not covered by these committees, members should write to: Sherman Ross, APA Convention Manager; Department of Psychology; University of Maryland; College Park, Maryland. The Chairman for each Convention Arrangements Committee is listed below.

Registration. Chairman: Allen O. Gamble; National Science Foundation; 1951 Constitution Avenue; Washington 25, D. C. APA members are urged to register for the convention in advance in order to expedite receipt of their convention materials. Upon arrival at the Registration Desk, preregistrants will only need to indicate their Washington address and will immediately receive an envelope containing their badge, *Convention Guide*, and other materials.

Please note that there are two types of Advanced Registration Forms. The APA Member Advanced Registration Form appears at the end of these notes. This form should be sent to: APA Hous-

ing; Washington Convention & Visitors Bureau; 1616 K Street, N.W.; Washington 6, D. C.

The Nonmember Advanced Registration Form appears at the end of these notes. The nonmember form should be sent to: Nonmember Registration; 1333 Sixteenth Street, N.W.; Washington 6, D. C.

Directory and Mail. Chairman: Thomas M. Magoon; Department of Psychology; University of Maryland; College Park, Maryland. A directory of members and guests registered, together with a mail box and bulletin board, will be maintained in one of the headquarters hotels for the convenience of those attending.

Information Desk. Chairman: Cecil P. Peck; Clinical Psychology Division; Veterans Administration Central Office; Washington 25, D. C. An Information Desk will be maintained between the hours of 9:00 A.M. and 5:00 P.M. each day of the convention. Personnel at this desk will provide such convention information as room locations and the time and place of scheduled events. In addition, they will make available information about desirable restaurants, local points of interest, and recreational facilities.

Few cities in the world offer as rich a visiting experience as does Washington. It is not only the nation's political capitol, but a world center of scientific, intellectual, and social activity of interest to psychologists and their families.

Special Events. Chairman: Arthur R. Laney, Jr.; Washington Gas Light Company; 1100 H Street, N.W.; Washington 5, D. C. Requests for luncheons and dinners for APA boards, divisions, committees, and other groups which are to be included in the July *American Psychologist* should be addressed to the Chairman of the Special Events Committee. All requests should include a statement of estimated attendance, time required, time and day preferred, and whether arrangements for meal service or cocktails are desired.

Divisions and other organizations desiring meal accommodations can contact the Chairman of the Special Events Committee for menus and prices. He will also accept tickets from groups for sale. The sale of tickets will continue until one hour before the function is scheduled to begin. The remaining tickets and the funds collected will then be turned over to the organization's representative so that last-minute sales may be adequately controlled. Individual organizations will be responsible for guarantees to the hotels.

The Chairman of the Special Events Committee is planning to schedule some programs of interest to psychologists' wives. Several ideas are now being considered. The committee would like to hear from the wives of psychologists concerning the kinds of activities that would be of interest to them.

The Special Events Committee plans to arrange for one or two convention lounges to be open to members and guests. Members are urged to use these facilities for informal get-togethers.

APA Day Functions. Chairman: Denzel D. Smith; Office of Naval Research; Department of the Navy; Washington 25, D. C. Sunday, August 31, 1958 has been designated as APA Day. Division program committees will not schedule any program for any part of that day. Arrangements are being made for a program of APA-wide interest for this block of time.

Exhibits. Chairman: Melvin R. Marks; Personnel Research Branch; The Adjutant General's Office; Department of the Army; Washington 25, D. C. For information concerning facilities, arrangements costs, etc. write directly to the Chairman of the Exhibits Committee. He would appreciate the names of potential exhibitors.

New films, film strips, or other audio-visual materials should be submitted to Elliott M. McGinnies; Chairman of the Committee on Special Film Presentations; Department of Psychology; University of Maryland; College Park, Maryland.

For further details on exhibits and audio-visual materials refer to the "Call for Papers and Symposia" which appeared in the January *American Psychologist.*

Public Information. Chairman: John L. Finan; The George Washington University; Human Resources Research Office; Washington 6, D. C. A press room will be maintained during the entire convention for the release of information to the public.

Placement. Chairman: Carole E. Bare; Psychiatric Institute; University of Maryland; Baltimore 1, Maryland. A placement office will be open from 9:00 A.M. through 5:00 P.M. during the entire convention, except for APA Day, Sunday, August 31.

Audio-Visual Services. Chairman: Curtis E. Tuthill; Department of Psychology; The George Washington University; Washington 6, D. C. This committee will make available audio-visual equipment where needed. Such equipment includes public address systems and projectors. Authors of ac-

cepted papers will receive a checklist during the summer on which to indicate their audio-visual requirements. Since these services are rather costly, participants are urged to consider the possibilities of handouts for demonstration materials.

Coordination of Voluntary Student Help. Co-Chairmen: Helen E. Peixotto and Walter J. Smith; Department of Psychology; The Catholic University of America; Washington 17, D. C. The administration of a convention requires a very considerable amount of help. To partially meet this need, we have, in the past, appealed to students to contribute their services. They have answered this appeal, and we look forward to their further services. The committee would appreciate the help of local department chairmen and of teachers in summer sessions in recruiting student volunteers.

Signs and Posters. Chairman: Allen R. Solem; Department of Psychology; University of Maryland; College Park, Maryland. The committee will prepare the signs required to locate the many convention activities. Also, this committee will work closely with the Audio-Visual Services Committee in developing nameplates for symposium participants.

Convention Treasurer. Max Meenes (Department of Psychology; Howard University; Washington 1, D. C.) has the exacting job of Convention Treasurer. He urges divisional officers to monitor their financial commitments with the hotels very closely to avoid confusion with those assumed by the APA Central Office and the APA Convention Arrangements Committees.

NONMEMBER ADVANCED REGISTRATION FORM

SIXTY-SIXTH ANNUAL CONVENTION OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION

Washington, D. C., August 28–September 3, 1958

Nonmembers of APA are requested to complete this form for advanced registration.

Please type or print:

Name: Dr. / Mr. / Miss / Mrs. ..... (Last) ..... (First) ..... (Middle)

Professional Affiliation: (to appear on badge) .....

.....

..... (City) ..... (State)

The following information is requested for the Convention Directory:

1. Expected date of arrival at convention.....

   Expected date of departure from convention.....

2. Divisional membership by number.....

When you arrive at the convention, pick up your badge and convention materials at the Advanced Registration Desk and give your Washington address to the receptionist. There will be no additional forms to fill out.

A fee of $2.50 must accompany this form. Make checks and money orders payable to: 1958 APA Convention. Please send this form, together with the fee, as early as possible to:

Nonmember Registration
1333 Sixteenth Street, N.W.
Washington 6, D. C.

(Copies of this form may be obtained from the APA Central Office)

APA MEMBER ADVANCED REGISTRATION FORM

SIXTY-SIXTH ANNUAL CONVENTION OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION

Washington D. C., August 28–September 3, 1958

Please type or print:

Name: Dr. Mr. Miss Mrs. ..... (Last) ..... (First) ..... (Middle)

Professional Affiliation: (to appear on badge) .....

.....

(City) ..... (State)

The following information is requested for the Convention Directory:

1. Expected date of arrival at convention.....
   Expected date of departure from convention.....
2. APA membership status:
   Fellow..... Member, Student Journal Group.....
   Member..... Foreign Affiliate.....
3. Divisional membership(s) by number.....

When you arrive at the convention, pick up your badge and convention materials at the Advanced Registration Desk and give your Washington address to the receptionist. There will be no additional forms to fill out. There is no registration fee for APA members, Foreign Affiliates, and Members of the Student Journal Group.

Please send this form as early as possible to:

APA Housing
Washington Convention & Visitors Bureau
1616 K Street, N.W.
Washington 6, D. C.

HOTEL RESERVATION FORM

The rates listed below for the headquarters hotels apply only if reservations are made through APA Housing; they can not be guaranteed by APA Housing if reservations are made after August 14, 1958. Reservations should be made as early as possible through:

APA Housing
Washington Convention & Visitors Bureau
1616 K Street, N.W.
Washington 6, D. C.

Please make all changes and cancellations through APA Housing.

	MAYFLOWER HOTEL			STATLER HOTEL		
	Number Available	Rate	Type & Number to Reserve	Number Available	Rate	Type & Number to Reserve
Single Room(s)	30	$ 8.50	.....	172	$ 8.00	.....
Double Room(s)	140	11.50	.....	137	12.00	.....
Twin Bedroom(s)	250	13.00	.....	337	12.50	.....
Suite(s)	40	24.00–37.00	.....	20	37.00–45.00	.....

Note: All rates subject to Washington, D. C. 3% Sales Tax.

Be sure to list definite arrival and departure date and time:

Date of arrival ..... at ..... A.M. P.M.

Date of departure ..... at ..... A.M. P.M.

Hotel Reservations will be held only until 6:00 P.M. unless otherwise specified.

Room occupants: (Be sure to give the names of all occupants and bracket those sharing a room.)

Name (Please print)	Sex	Address	City	State
.....	.....	.....	.....	.....
.....	.....	.....	.....	.....
.....	.....	.....	.....	.....

The APA Housing will confirm your reservation.

Mail Confirmation to: (Signed)..... Address ..... City..... State.....

Note: Additional hotels, together with a description of the accommodations and rates, will appear in future issues of the *American Psychologist.*

(Copies of these forms may be obtained from the APA Central Office)

announcing three

THE BEHAVIOR OF MAN:

by **Karl U. Smith**
University of Wisconsin

This outstanding volume presents an objective account of man's behavior as psychologists today understand it. The organization is unusually teachable in that it capitalizes on the student's customary interest in adjustment to bring him to intensive study of the experimental areas, with which the main portion of the book is concerned. THE BEHAVIOR OF MAN demonstrates, too, a most effective new conception in textbook planning. Clear, scientific writing is strongly reinforced by highly original and abundant illustration—hundreds of drawings in two and four colors and a number of sequential photographs depict psychological facts, mechanisms, and experimental sequences.

Workbook: Each chapter of the workbook provides a carefully planned course of study for the parallel chapter in the text: introduction and definition of terms, questions based on illustrations from the book, a self-test quiz of about 50 questions (true-false and completion), suggested experiments and a list of pertinent readings.

528 pages, $6.95

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Another landmark in American psychology from one of the field's most prominent figures. The text is largely concerned with theories of motivation and learning, endeavoring to deal fairly with opposing theories in this area and show what each implies. Complex ideas are clearly organized and aimed at stimulating class discussion. Frequent use of concrete experimental material adds vividness to the book and will particularly assist junior and senior students.

Published in March

Henry Holt and Company, Inc.

1958 titles in Psychology

An Introduction to Psychology

and **William M. Smith**
Princeton University

Additional Teaching Aids

1. A 64-page TEACHER'S MANUAL (free to teachers) contains discussion questions, a mid-term and final examination, teaching suggestions, a list of films on psychological topics, and an excellent bibliography.
2. Hundreds of additional QUESTIONS have been compiled by chapter and will be supplied upon the teacher's request.
3. The Manual's mid-term and final EXAMINATIONS are available separately at 10¢ each, upon the instructor's authorization.
4. An IBM SCORING KEY is provided for each examination. Scoring may be done manually or by IBM equipment.
5. IBM ANSWER SHEETS for these examinations are provided free up to the number of tests purchased. Additional sheets may be ordered in lots of 100 at $4.00 per hundred.

READINGS IN SOCIAL PSYCHOLOGY, 3rd Ed.

Eleanor E. Maccoby, Theodore M. Newcomb, and Eugene L. Hartley

Since its initial publication in 1947, this text has been used in hundreds of colleges and universities as a basic text and an indispensable source of important research reports. The new edition will retain those articles of recognized importance and add a number of articles written especially for this collection. There is coverage of such diverse topics as the "new look" in perception, group dynamics, communication and persuasion, prejudice, and cross-cultural comparisons of child-training practices.

May

383 Madison Ave., N.Y. 17, N.Y.

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